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Psychological Bulletin

SOME RAPPROCHEMENTS IN CONTEMPORARY PSYCHOLOGY[1]

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I.

To introduce my theme tonight let me recommend to you some simple experiments. Suppose you hand to a blindfolded person a pack of cards with different-shaped holes punched in them and ask him to sort them as fast as he can merely by 'feeling' these holes. You are likely to find that he will sort them faster if you contrive to allow some diffuse light to enter his eyes. The retinal stimulation will facilitate the cutaneous discrimination (33). Again, suppose you give your human guinea pig (and yourself) the task of determining just when he can discriminate 2 closely printed black squares as 2 instead of 1; and then, while he is at this visual task, suppose you subject him (with proper warnings) to stimulations in some very different modality (by blowing a whistle, holding an odorous bottle under his nose, or pressing down upon his hand). Under such conditions he will show an acuity of vision clearly superior to what he can show when he is not being stimulated in these extraneous ways (20). And what is more, you will find that the effect which a sound will have upon his visual acuity will depend upon the precise time interval between them (13). One more illustration! With fine scientific impartiality turn guinea pig yourself. Gaze awhile at a colored disc, then remove it and inspect the character and course of your after-images; and while you are at this have an assistant sound interrupted tones of different frequencies. Your after-images

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will often flicker in a curious synchrony with the interruptions of the tone. They may even change their very complexion in some rude conformity with pitch changes in the tone, appearing brighter or darker, colder or warmer, clearer or muddier, when the tone is higher or lower (73).

Shades of our predecessors with their air-tight sense modalities! They did, to be sure, recognize the synesthesias, colored-hearing, number forms, and the like. They recognized that there was some sort of technical justification for certain popular phrases, as that the off-key tenor is singing 'blue' or 'sour,' or as when one confesses to a 'dark-brown taste' on the morning after the night before. Such curiosa were, however, curiosa. In these latter days, on the contrary, such rejected stones have been used for foundational places in the structure of theory. They are claimed to point toward a deeper genetic unity of the senses.

It shall not be my intention tonight to follow up these lines of research nor to argue their interpretation. I suggest them only as an analogy. These interactions across modalities that threaten to break down the conventional pigeonholes of the senses typify, it seems to me, many current developments of these latter days. They typify certain gravitational tendencies that are offsetting to some degree the tangential directions inevitably taken by most scientific research as conducted by this individual and that. A presidential message often takes the form of an address on the state of the Union. It shall be my purpose on this occasion to sketch some of the currents that mark not 'the psychologies of 1925' nor 'of 1930' but 'the psychology of 1938.'

II.

Our friends, the philosophers, have been gently 'ribbing' us of late. As they put it, psychology once boasted that it had come of age as a natural science when it thought itself emancipated from its embarrassing entanglement with philosophy. "And now look!" say they, gleefully, "Psychology is returning almost hat in hand to ask for philosophic reconsiderations of her science." But—they should be reminded—the reconsiderations in progress today are not epistemological, as they were before: they are logical; that is to say, methodological. They are not concerned with such questions as why the mind has a body, but with problems of the most effective methods of investigation.

True, there are still some very active survivors of a school of

psychology that has stood for a psychophysical parallelism as the best rack on which to hang their data; but the overwhelming majority of psychologists deal with the same data and consider it needless and irrelevant to set up a dualism of the mental on the one hand and the bodily on the other. Is this dualism anything more than a dualism of artifacts? In Santayana's phrase, it is "a clumsy conjunction of an automaton with a ghost." The Cartesian dualism, as it is called—though of course older than Descartes'—reminds me of Humpty Dumpty, for, once a split was made between a so-called mental and a so-called material world, then all the king's epistemologists and metaphysicians could not put them together again. And so I move the previous question. I ask you: Is this a legitimate distinction in the *first* place? Certainly it has turned modern philosophers' hair gray, by being insoluble and—like a One-Eyed Connolly—by crashing the gates at all their parties. It is still the Unwelcome Specter at so many philosophical feasts.

Let us look at that juncture in history that led to the enunciation of this dualism at the hands of Descartes. His fancy taken, as the story reads, by the mermaids and Neptunes and other creatures in the fountains at Versailles that sprang up to confront the visitor whenever he trod on this stone or on that, his thought leaped to a stroke of genius as he likened to the movements of these man-made forms the activities of man himself. Behavior was as mechanical as the newly discovered circulation of the blood. Did not a man act when acted upon, return smile for smile or blow for blow, cry aloud when hit, or gurgle when soothed? His actions were, then, reflected back; they were reflections, or reflex actions. And with this as the key notion a whole machinery of human behavior offered to unfold itself. Like animals, men were automata.

But in Descartes' day the mold in which intellectual discourses had been cast for many preceding centuries had been pretty exclusively that of a man as a tenement of clay, harboring an incorporeal agent. Some place had to be made, so it seemed, for this soul. The result: a dualism, a 'clumsy conjunction,' that for its rationalizing forced later thinkers into fantastic theories about a 'preëstablished harmony' or 'two attributes of a common substance' or a steam-whistle 'epiphenomenalism,' and so on with the range of insoluble brain teasers that form so much of modern philosophy.

Well, for psychology today, this dualism is meaningless. The twin assertions that psychology is the study only of conscious experience and that it is the study only of externally observable motor

processes are both wide of the mark. Let me illustrate with some current concepts. What about Binet's intelligence: is it made of mental stuff or bodily stuff? What about McGeoch's retroactive inhibition? Ach's *Einstellung?* Köhler's insight? Lewin's valences? Freud's complexes? Maier's reasoning? Krechevsky's hypotheses? Floyd Allport's attitudes? To wrench concepts like these out of their biosocial settings and subject them to the question of whether they belong to the psychical world or to the physical world must strike us as more than amusing: it is a downright impropriety.

III.

Psychology, I repeat, has not turned back to philosophy as epistemology but to philosophy as the study of method. In this respect psychology is, of course, only one more of those natural sciences whose devotees have become concerned about their presumptions and their operations. While the physicists, perhaps, have been the most alive to the need of reëxamining scientific methed, workers in other fields have become method-conscious, too, with the mathematician often serving as pro and doubling as caddy. Shall I remind you of a few evidences of this new interest on the part of psychologists?

1. One thinks of operationism, for one thing. Previously enunciated in the field of physics (by Bridgman), it has been brought to our attention by Stevens, Boring, Kantor, McGeoch, and Tolman (**63**, **64**, **8**, **35**, **44**, **45**, **66**) as a principle of rigor that we should observe as we frame our own concepts. We should use terms only with the meanings given them by the operations and observations which give rise to them. The controversy over 'instincts' that used up so much of our ink 10 years ago is a beautiful case at hand. Had we restricted that term to the meaning that on such-and-such occasions people will act thus-and-so without any apparent previous experience in the matter, *i.e.* 'instinctively,' and had we not gone on to dispute whether people have tucked away inside of them this or that 'instinct' as a hidden spring, we would have saved ourselves pyrotechnical displays that sorely distracted honest workers from their honest observations of what people honestly do do.

The value of operationism does not lie in any novelty claimed for it. It was in operation—pardon the pun!—back in the 1880's, when Ebbinghaus stated propositions strictly in terms of the measurements he had employed. You will recall how his marvelously clean-cut experimentation led to generalizations on such questions as the following: "Rapidity of learning series of syllables as a function

of their length" or "Retention and oblivescence as a function of the time." What neater example could we ask of the positivistic spirit—which is, I take it, only another name for an unusually stubborn insistence upon our not going beyond the facts we can observe into speculating about ultimate existences and causes.

2. In something of the same positivistic vein Hull (**23, 25**) is urging us to look especially to the systematic character of our thinking. Of course, one mark of scientific procedure is its empirical character; but investigations that are 100% purely empirical are random, haphazard, and fragmentary. If theories without data are empty, data without theories are blind. Experimentalists, we are reminded, would do well to point up their researches by recognizing more explicitly what assumptions they are making, by formulating them articulately in definitions and postulates. From these postulates let them proceed rigorously to deduce theorems which can be put to the experimental test of verification. Finally, Hull says, if these theorems are carefully worked out so as to be consecutive and interlocking, the eventual product will be a rigid scientific system buttressed on all sides. You can see that this robust methodological reform is designed really to produce a double reform: it would stimulate the experimentalists to more formal articulation of their problems; it would lead theorists to scrutinize their doctrines for precision, consistency, and parsimony.

3. Not all latter-day revisions of the psychologist's methodology are positivistic. There is the one advocated by Lewin, for instance, in which he rejects what he calls the Aristotelian way of thinking for the Galilean (**9, 40**). One characteristic of the former is illustrated when we think we have understood a thing by assigning it to a class: thus the error of the social stereotype; thus the false sense of having deeper knowledge when we have assigned a psychological phenomenon in question to the genus "perception," "emotion," and the like. The Galilean logic *per contra* seeks to understand a phenomenon by assigning it to its "field" and determining the relations in which it is embedded. From the now abundant writings of the Gestaltists, we should be familiar enough with the latter concept and its emphasis upon dynamic interpretations.

Another point of contrast between the 2 kinds of thinking, according to Lewin, is that whereas the follower of Aristotle puts his reliance upon frequency of occurrence and upon statistical validating and standardizing of particular phenomena, the follower of Galileo rejects as scientific instruments these beds of Procrustes that

lop off the distinguishing marks of each individual case. Indeed he emphasizes their very individuality. But let me develop this point later.

I have been, perhaps, to unnecessary pains in reminding you of these various renewals of interest by psychologists in the philosophy of science. But questions of scientific procedure can hardly be inappropriate just now when the unlaid ghost of occultism has been squeaking its gibberish over the radio, and the general public has been invited by newspapers to sit as judge of a technical question in a technical field.

IV.

Psychology in the 1930's is developing more rapport with *biology*. And it is high time! To be sure, physiology has been a sister science for decades; but, if you please, one may be a physiologist without being a biologist. Too long have we been between the horns of a false dilemma: too long have we supposed that all our subject matter must be cast either into the physicist's categories or into the subjectivist's. The falsity of this dilemma is becoming apparent in several ways.

Consider, for example, Tolman's refreshing attempt to restate certain observable biological phenomena in ways that avoid implying gratuitous entities (65). When a rat is said to manifest a "goal-expectation" or a "means-end-readiness," this is something the experimenter can, if he only will, directly observe. Anyone sufficiently unburdened of preconceptions can see that the rat *is* hell-bent for the food box, with no questions asked about its subjective experiences or about the particular pattern of its reflex thrusts.

Let me not be misunderstood, now! This effort at redescribing animal behavior is noteworthy not because it is behavioristic but because it is an attempt to identify certain biological phenomena as they are originally, naïvely, immediately observed. May it not be that much of our difficulty in accepting such concepts as "purpose" in our predominantly objective American psychology is traceable to our well-known bias for description in terms of the physicochemical sciences only? What cannot be expressed in terms of the physicist cannot be expressed objectively at all, we seem to think. But let any three-year-old child do the observing for you. When his cat comes running in answer to the call of "kitty," he knows that it is running *to him*. When his dog whines at the door he knows it "wants out." And in neither case does he (as Avenarius used

to claim) project or introject into the mind of the animal any duplicates of what he himself would have experienced under analogous conditions. Such a notion is altogether too sophisticated. No! I repeat, the child is noting a simple fact of biological order.

It would be a fascinating task, if time and your patience permitted, to elaborate on biological concepts of broader sort that are leavening the loaf now. But under the conditions tonight I dare only mention some of them without attempting to trace specifically their influences on our science.

There is, for instance, the newer viewpoint on the old nature-nurture controversy. If the germinal cell—nay, even the gene—is in some sort of environment as far back as we can trace it, and if—to look the other way—certain changes that occur in a person as he lives his years of life are largely independent of the conditions of that life, then we do well to cease following the *ignis fatuus* of the old debate. Taking up, rather, the leads of Jennings (**31, 32**) and other biologists, we do well to cease invoking the concepts of "heredity" and "environment" in an absolutist manner, and, bending our efforts to the mapping of *development*, to inquire in each particular case: Just how much of this observed change can we trace to extrinsic factors operating recently or now; just how much to intrinsic ones?

The *organismic* emphasis is, of course, one of the outstanding marks of psychology of recent years. For decades, only lip service has been paid to the notion of man-as-a-whole; and even Dewey's warning in the 1890's against overworking the reflex-arc concept was frequently cited only to be neglected. It seems to have taken the incisive work of a biologist (**14**) on his salamanders to arouse us to an acute appreciation of what other zoologists had been observing (**30, 57**) but which we had been overlooking in our human specimens: that the particular organ or the particular act is dependent upon the dynamic interplay of forces within the total organism—or the total personality.

Once we have envisaged the organism as a true biological unit and the personality as a true psychological unit, inevitably we find ourselves emphasizing the concept of *adjustment*. Descended along the British line from Spencer and Darwin, the term had been in rather glib use by camp followers of the functional school of psychology when that was on the march. Lately it has been dusted off and put to more illuminating uses in the modern revival of an insight as old as Hippocrates. Whether we call it "regulation" after some

biologists (12, 30) or "homeostasis" after others (11), it is a key idea linking together such disparate human functions as a reflex retraction from a needle, a redoubled concentrating of attention to offset a distracting noise, an hysterical headache when unwelcome tasks impend, and the temporarily pepped-up condition of him who has lost sleep (53).

Finally, I should mention the notion of *emergence*. Though much the same general conception had appeared a century ago in Mill's "mental chemistry" and later in Wundt's "creative resultants," it has in the last decade been revivified by certain biological writers (48, 69). Applied by psychologists to the individual, it is made impressive and convincing by the undeniable bursting into bloom of so many human traits at just this age or that—traits quite unpredictable from what we can see of their antecedents only. This blossoming out of quite new things in the individual life is a central character of humans as living beings, for the unfolding drama of maturation can claim only distant analogues in the nonliving world. And as in his biographical maturing, so also in his thinking. As Browning put it, though less scientifically than did Mach and Ehrenfels:

> "But here is the finger of God . . .
> And I know not if, save in this, such gift be allowed to man,
> That out of three sounds he frame, not a fourth sound, but a star."

Originally, we are told, "psyche" meant "the animating principle of life." And so today, when concepts from biology are reanimating our "'ology of the psyche," we are regaining ancient wisdom.

V.

This development of understanding between psychology and other fields of knowledge is to be taken, I feel, as one sign of the times. It is an expression of the *Zeitgeist*. Today the centrifugal tendencies inevitable in scientific excursions into the unknown are counterbalanced by centripetal; and though we still enjoy the hackneyed phrase that the specialist is he who learns more and more about less and less, we are also compelled to recognize that the good scientist is he whose vision is not limited by barriers erected at the demarcation lines between the 'ologies as set up by Aristotle in the Fourth Century before Christ.

But fully as striking, I think, are the expressions of this *Zeitgeist* to be discerned in recent developments within psychology

itself. If we take the field of *learning*, for instance, we cannot fail to be impressed with the fact that the more thorough the study the broader the theoretical interpretations. As I have already tried to show this elsewhere (**15**), I will be brief.

There have been 3 predominant viewpoints on how people learn: the British-originated trial-and-error, the Russian conditioned response, and the German insight theories. After having been exploited separately and even in separate laboratories by us Americans as if distinct and irreconcilable, they are turning out to be mutually supplementary. Their differences had been magnified unduly by the particular experimental setups used; and nowadays the learning process under observation clearly overflows these artificial boundaries. Let me mention a few of the most important principles common to all learning—whether it be a chick, 'chimp,' or child floundering about in attempts to escape from a maze, or a sheep or sophomore withdrawing from shocks or air puffs, or a man or monkey forced to devise a circuitous way to obtain forbidden fruit. (1) In every case the learner must be in some way motivated; (2) but he meets some obstruction; (3) he displays his repertoire of acts (or ideas of acts); and (4) if a given act (or idea of act) stands in a fruitful relationship to the objective it will, immediately or eventually, become the established line of action.

This is a formal analysis *ex post facto*. But the reader of current literature can hardly fail to be impressed with the increasing number of experimental studies deliberately set up to show the general point. I mention some scattered samples. (1) Wertheimer's insight principle that a tone depends upon the pattern in which it is found has been demonstrated with Pavlov's conditioning technique (**26**). (2) In the memorizing of nonsense syllables, forgetting has been shown to be identical with experimental extinction (**52**); and remote forward associations, with trace conditioned reactions (**39**). (3) A response learned in the Thorndikian trial-and-error manner by a rat shows the same retention curve as the conditioned eyelid reaction (**72**). (4) And it has been shown that a single equation of the learning curve, with a single set of assumptions, can be made to represent both trial-and-error and insightful learning (**70**).

VI.

To turn now from a highly experimental field to one in which interpretations play the leading rôle, let us look in on developments in the psychology of *Personality*. I suggest that interests are con-

verging there which have arisen from some 3 or 4 independent sources.

One, which I will dub the *descriptive* or structural viewpoint is that ancient and modern concern with the cross-sectional pictures of man's make-up. The eighteenth-century faculties gave place to the nineteenth-century functions and processes of the mind; and these were knit together by the concept of the Self. Within the experience of most of us here, the Self was the concluding topic in any systematic presentation of man. It was an expression of the man *in toto* again. But how to study man *in toto*? We have tried somehow to capture the total man by viewing and measuring his various "traits"—his focalization of interest, his ascendance over others, his emotional stability, and so on up to the 18,000 traits exhibited from the literature by Allport and Odbert (**3**). Today one trend is toward a simplification of these through inspection or by the more rigorous procedures of factorial analysis (**19, 49**). Questions concerning their nature, their origins, and their interrelations are still current jousting fields in which many a lance is being shattered (**1**).

The second, which I would call the *psychopathological* interest in personality, originates principally in the dramatic studies of Charcot, Janet, Ribot, Prince, and others on patients with hysterical and other forms of dissociation. With the stemming off of psychoanalysis the center of gravity shifted to motivational problems, to the sources of the splitting. More and more distinctly the central problem has become that of how the individual deals with conditions that frustrate him: the adequacy of his personal adjustments.

This point of view is a more dynamic one. Lewin (**40, 41**), for example, has been an active proponent of the view that what a person is and does is determined by the field in which he lives and moves and has his being; and it is becoming an interesting problem just what psychologists are going to do about his vectors and topological analyses. Are these, as a certain acidulous critic remarked the other day, just a lot of map drawing, or are they going to furnish a basis for predictive work?

The clinical approach to personality is dynamic in more general ways as well. What are the causes of handwashing compulsions, weeping spells, rage fits, general cussedness, and other emotional extravagances? Some say they are to be traced back through the biography of the individual person, by way of so-called mechanisms, to earlier episodes in which he had been frustrated and when he took the wrong way out (**5, 56, 60**). The influence of psychoanalysis is,

of course, tremendous here. It is particularly revealed in mental hygiene.

A third direction from which personality has been approached is the *physiological*. Endocrinology, though certainly needing to be saved at times from its friends, has fired the imagination with its pictures of patients in whom profound changes of all sorts have been wrought (**22, 71**). Equally dramatic are the personality alterations now reported from many quarters resulting from heroic treatments with raging fevers and the very shocking use of protein and of insulin (**59, 76**). Examinations of blood, saliva, and urine have established several parallels between emotional status and body chemistry (**54, 55**). And even electroencephalograms have been found to run consistently differently for Jones, for Smith, and for Brown (**18, 67**).

Finally, we must recognize the impact of the newer studies of the *cultural anthropologists* who get quite outside the individual person and look at his social environment. The individual personality, they say, is shaped by the cultural pattern. Explorations of savage cultures (**6, 7, 43, 46**) are reinforced by studies upon American folk caught in different physical surroundings (**37**), in showing the great power of the "grip of the group."

Now to return to my main thesis. These interests, originating as they do quite diversely, are converging in present-day work on the psychology of personality. The newer textbooks show this (**2, 56, 62**). And in the reports of clinical studies of individuals which now abound in our literature, they are all, perforce, given attention.

VII.

It would be easy to multiply these thumbnail sketches of psychological fields or psychological problems of the day whose richness of subject matter is drawing together investigators of originally varying interests. However, I prefer to draw upon what remains of your patience to present what has struck me as the most impressive of these contemporary developments. I refer to signs of rapprochement between *the experimental* and *the clinical attitudes*. Here, certainly, has been one of the serious cleavages in our profession, past and present.

It has been conventional for us to point to the date of 1879 as marking the definite emergence of psychology as science. Was not that the year a psychological laboratory in name as well as in function became established at Leipzig? Or, if you wish, we could set

the fateful date back a few years to when Helmholtz brought forth his *magna opera* to show the fruits of experimentation applied by one who was primarily a physicist to problems interesting the psychologist, or still earlier to when Weber and Johannes Müller had done much the same as physiologists. In the second and third quarters of the Nineteenth Century, then, and especially in Germany, the study of the mind—whatever that means!—became established as primarily a field for experimental exploration. It was miraculously born out of philosophy by a double impregnation by physiology and physics. Speculative activity continued, of course—Wundt contributed his full share of speculation!—but it was sobered by contact with laboratory procedures.

This development was, of course, natural enough. If there is one thing in which modern science is to be differentiated clearly from that of the ancients, it is the method of the experiment. It is the method whereby we study a certain kind of happening not by standing by until it happens but by making it happen, and so, in the very business of making it happen, discover the conditions under which its happening does or does not occur.

The dominance of academic psychology by the laboratory has been manifest here in America. Run through descriptions of courses offered in the colleges. You will see that they are given over largely to discussions of sensory and imaginal phenomena, spatial and temporal perception, affective tone, association and memorizing, and reaction time. Now, where did these topics come from? The amount of space and emphasis and detailed analysis was dictated by their prominence in the laboratory. To be "factual" the teacher and his book had to be limited to the experimentally factual. Or so it has seemed. It has been common to measure the progress of psychology in America by counting the increasing number of laboratories established. "Psychology" in this view of the matter has been identified largely with "experimental psychology."

In high contrast to the experiment is what I am taking the liberty of calling "the clinical method." I use the name—for want of a better—to include all those procedures in which the peculiar make-up of the individual person is the primary subject matter, and in which his particular career in some sense is considered on its own account. What are the factors in Smith that make it impossible for him to hold a job, or to drive an automobile safely, or to ride through tunnels without fear, or to live on 24 hours a day? What vocation can be recommended to Jones? Should his daughter be promoted

ahead of her school grade? Let us call all of these 'clinical questions.' The contrast was implied when, not long ago, I heard a psychiatrist, in the course of an after-dinner speech, remark that at the start of his career he had made a pretty thorough study of general and experimental psychology, only to be disappointed at finding that psychology could not tell him anything about *people*. Is this not an evidence of what Gordon Allport has recently called the problem of the individual in all science (2)? A natural science, being interested solely in educing uniformities of nature, does not find the individual an object of concern save as it illustrates an old law or suggests a new one. Parenthetically, I must say that I am not quite sure of this in all cases: consider the astronomer's interest in Aldebaran or in Arcturus or the statistician's interest in the individual deviate. But at least it applies to much naturalistic observation and to all true experimentation, for there the specific instance, once it has been noted, is tossed aside like a squeezed lemon, its juice having been extracted and compounded with that of numerous others.

But we must bring into sharper focus the differences between the experimental and the clinical approach to the study of man. In the former the aim is to analyze the specific contingent relationships, and (true still to the fundamentals of Mill's methods if not to his notion of "cause") to hold constant or negligible all other variables than the one or the two that form our particular concern. By definition, almost, experimentation assumes the isolation of a phenomenon from all incidental and simultaneous cross-phenomena. We study such things as the various factors that modify a *typical* human being's reaction time, or the relationship of time interval to loss of ability to reproduce a memorized passage by adults *in general*, or the dependence of the variable of problem-solving ability upon the age variable in the *average* child. Note 2 things: (a) we do not isolate the individual person *qua* individual; but (b) we do isolate the particular function or phenomenon in question.

Quite to the contrary, when we make a clinical approach to the study of John Doe, (a) we are interested in John Doe not as a specimen but as a unique thing, to be considered *sui generis*, whose past, present, and future may be our prime concern; and (b) we are interested in him *in toto* and in his natural and social habitat. The experimentalist's allegiance is to the discovery of natural laws, and he can set up his laboratory situations when and as he pleases; the clinician's allegiance is to the human 'case,' and the need of some decision on that case may be importunate and vital.

This distinction does not correspond, now, in any precise way with divisions of the psychological subject matter as represented, say, by different sessions of our Association program or by the courses listed in college catalogues. Child, Abnormal, Educational, Industrial, Social, Statistics, Personality, Memory, Perception, Learning—most of these labels cover fields in which the activities of psychologists do not fall exclusively into the one or the other of the 2 classes. What I am trying to point out is not a difference of subject matter so much as a difference of attitude, viewpoint, and ultimate aim.

There is a difference of method that follows frequently from this difference in objective, though not by any necessity of logic. When one is in the laboratory, seeking to discover and establish general laws, he emphasizes quantitative precision in his data, and he refuses to draw any conclusions that are not positively and indisputably compelled by them. But when one is in the consultation room examining a person, he often finds it appropriate, indeed necessary, to use his best personal judgment, his insight, his intuition (in this particular meaning of the term). The difference is analogous to that between the methods of the medical man of the physiological laboratory and those of the medical man in private practice.

The historical development of the experimental attitude in psychology I have sketched a moment ago in a few sentences. What has been the history of the clinical attitude? Being less simple in origins and lines of development, this may demand a somewhat longer sketch. But I will skim it rapidly.

The scientific interest in the individual John Smith or Sally Jones seems, in modern times, to have begun in an interest in the abnormal John Smith or Sally Jones. As the clouds of superstitious explanations of their peculiarities were being dissipated at the turn of the Nineteenth Century, serious students, like Rush, and still better, Pinel and Esquirol, began to look upon the insane as natural objects in a natural world. This impersonal natural-science viewpoint was supplemented also by interest in the welfare of these individual unfortunates. A clinical attitude was taken. This ripened later into a full-fledged psychiatry in Germany at the hands of Griesinger, Krafft-Ebing, and Kraepelin, who increasingly emphasized the study of the etiology, course, and outcome of the patient's illness.

In France, meanwhile, patients with milder mental disorders were being studied by such men as Charcot, Janet, Ribot, and Binet.

While the investigators did set up such things as hysteria or suggestibility or disorders of memory as generic problems, they saw them against the biographical background of each individual they studied. They were studying abnormal people.

I hardly need more than mention at this point the Freudian and other psychoanalytic schools that branch originally from the French line of work just mentioned. Of all the schools of clinical interest they serve to present most clearly the extreme antithesis to the experimental approach.

Quite a different source of interest in individual persons as such had appeared in England. Galton applied that wide-ranging spirit of inquiry of his to the study of how John and Sally differ, and upon what these differences depend. Elaboration of statistical devices for analyzing and describing these comparisons, by Spearman, Pearson, Kelley, Thurstone, and others, was of course to come now faster and ever faster. And the exploration of individual differences for pedagogical and for industrial objectives at the hands of such as Meumann, Claparède, Stern, and Thorndike is a part of this picture. But when all is said, differential psychology has its eye primarily upon the individual person, which is obvious enough when we realize that a most important practical outcome is the field of tests and measurements.

These clinical sides of psychology have engaged the attention of the layman so largely that for him "psychology" is likely to be synonymous with "psychological guidance," with "psychological testing," or with "mental hygiene." It is interesting to observe that while academically "psychology" is identified largely with experimental psychology, popularly it is identified more with clinical psychology.

The contrast which the clinical viewpoint has been opposing to the experimental in contemporary thought is particularly well pointed in the fields of characterology and typology. Of the making of types there is no end, especially among German and Swiss psychiatrists. There is, for instance, Kretschmer's division of his patients (and, by extension, of mankind generally) into the pyknic, asthenic, athletic, and dysplastic types (38). There are Jaensch's B-type, T-type, and various intermediates (29). There is Jung's division into the introverted, the extraverted, and subtypes (34). There is Rorschach's separation into the coarted, coartative, introversive, extratensive, and ambiequal (68). Broadening our survey, we see many

other classifications drawn up, based on handwriting characteristics (36), on predominating interests or values (16, 61), or what have you.

Typologies seem to rest upon 2 assumptions. One is that we can characterize the personality of any particular person by spotting some key trait or trait pattern that is central and basic and revealing. Is this, in fact, possible? The answer will depend upon which group of psychologists you interview. Certainly there are many characterologists, especially in Europe, who claim that we can. Their evidence is of the clinical sort. Certainly there are many personality-psychologists, especially in America, who insist that we cannot. Their evidence is produced by the quantitative methods of the experimentalist.

A second assumption of a typology is that in respect of a trait in question people will tend to fall into more or less demarcated groups. Now, there has been conspicuous lack of success by the more statistically-minded American investigators whose populational surveys fail conspicuously to find any but the unimodal forms of distribution. On the other hand, the typologists say it is much less important to classify people in the mass into neat stacks and piles than it is to give this fellow or that a simple characterizing name or symbol that will be revealing to one gifted with the proper intuitive understanding.

We obtain a bit of insight on this moot point, I think, when we bear in mind a difference between the kinds of human subjects considered by the German and Swiss psychiatrists who find their types and by the American psychologists who do not. The former deal almost wholly with abnormal patients, the latter with college or other normal populations. Is it not highly probable that there are tendencies for certain traits (physical, temperamental, intellectual, and all) to become clustered in certain patterns, but that in a general population such tendencies are obscured and are brought to light only after the selective action of hospital commitments?

The clearest antithesis to the academic experimental psychology, however, is to be seen in psychoanalysis. Here we have explicit denials and even challenges to all that is standardized or quantitative or isolated or cross-sectional. Nothing could be more repugnant to the analyst than the idea of putting a patient in a situation prearranged to fit a set of specifications, then of considering only one little aspect of his behavior as it is manifested just now in the laboratory and in disregard of his everyday life among people, and then of totaling up points and dial readings to arrive at a metricized score.

And not only is his methodology different; his ideology is different, too. He is interested first of all in the patient before him on the person-level, not on the behavior-segment-level. This is not the appropriate time to appraise the arsenal of complexes and mechanisms with which the analyst equips himself, nor to estimate judiciously the importance of his constant admonition, "Cherchez le motif!" It is sufficient to recognize him as the most extreme protagonist of the thoroughgoing clinical attitude toward human beings.

For a little while now I have been trying to present to you a difference in fundamental motivation and attitude to be observed among contemporary psychologists. With all due allowance for interpenetration they still remain recognizably distinct; so that if we were to plot on a graph the frequencies of psychologists to be found at the different degrees between the hundred-percent experimentalist and the hundred-percent clinician, we would find our professional colleagues distributing themselves in something of a bimodal curve. And—more's the pity!—the 2 subgroups have tended to work in independence, not to say in direct antagonism. Particularly is this true when the clinician's background of interest is more of the medical sort than of the educational or industrial.

However—and this is one of my principal themes tonight—we can discern at the present moment *signs of a developing rapprochement* even here.

1. First, a formal sign. Less than 2 years ago 3 divisions of the National Research Council (representing Biology and Agriculture, the Medical Sciences, and Anthropology and Psychology) organized an interdivisional committee under whose auspices conferences have been held on experimental problems in neurotic behavior which have been participated in freely both by experimental psychologists and by psychiatrists and psychoanalysts. These round-the-table interchanges of comments have served to demonstrate how the various interests represented are, after all, supplementary and mutually stimulating, so that programs of coöperative research can be organized to some point and effect (74, 75).

2. The most clear-cut program combining laboratory and clinic interests is the development of the study of experimental neuroses in subhuman species at Hopkins and at Cornell. You will remember Pavlov's account of the incidental setting up of a neurosis in a dog which was being trained to make a difficult conditioned discrimination. In America, Gantt (17) has been confirming these findings with some of his dogs; Liddell and his co-workers have been doing

the same with the pig and sheep (4, 42); and still others are having some success with cats, birds, and rats. If it be granted that we have here a condition similar to that of neurosis in the human, a vista of research that is highly promising is certainly opened up. The neurotic animal may serve as a standard preparation, so that under experimental controls the various conditions that lead to or facilitate or retard the breakdown can be isolated, and also the various therapeutic attempts can be tested for their efficacy.

3. Something of an entente cordiale between the experimental and the clinical is to be found in the program of the Harvard Psychological Clinic under Murray (51). Some 50 subjects of college age have each been studied in a protracted series of sessions, by psychologists, psychiatrists, physicists, criminologists, and others, each worker acting in turn and independently. The sessions included many kinds of tests of abilities, of interests, and of motivation, as well as prolonged interviews bringing out biographical materials and present personal problems. The spirit and aim of this whole project constitute an experiment—one in which the check and double-check of datum with datum were employed not so much with an eye to understanding and advising those 51 individuals as to putting the whole procedure to an *a posteriori* test. But there was also much experimentation—not simply testing—in the procedures of some of the sessions. These may be linked with other investigations by Murray, Rosenzweig, and others, who have experimentally analyzed personality mechanisms and even have set up quasi-neurotic phenomena in normal subjects. Repressions, projections, fantasies, compulsions, hallucinations, wishful thinkings, and the like, have been induced under laboratory conditions permitting a high degree of control and even measurement (50, 58).

4. Another occasion making for mutual understanding has been afforded by Hull's seminar at Yale. In the course of its very comprehensive program attempting the integration of all the social sciences, a long series of sessions has brought together in weekly discussions such right and left extremists as reflexologists and pychoanalysts (24). Prophecy was there fulfilled; for it is written that the lion and the lamb shall lie down together—though I am not sure which is the lamb. At these meetings the discussions turned on the more crucial concepts in psychoanalytic doctrines. These were illustrated and analyzed, and attempts were made to rationalize them in terms of the well-accepted laboratory principles of habit forming as seen through the spectacles of conditioned-response devotees.

5. Is it not a significant straw in the wind that at the 1937 meeting of this Association a session of experimental papers was devoted explicitly to Conflict and Frustration, and that many other such papers have appeared in other sessions last year and this? And further, that panel discussions have been devoted to the Experimental Approach to Psychoanalytic Concepts last year and to Frustration this year?

6. In Iowa and Kansas still another project of reconciling psychoanalytic and experimental principles has taken shape in the attempt by Lewin and by J. F. Brown to apply topological concepts to deviations in behavior (**10**). Doubtless it is a bit startling to you and me to realize that this involves adhering to a dictum of topology that only present facts can influence behavior, not the facts of yesterday nor the facts of one's childhood. Yet whatever our qualms on that score, the contributions of this school to experimental procedures—without instruments of brass yet nonetheless experimental—for the quantitative analysis of all sorts of problems of human motivation are certainly promising. The resuming of interrupted activities, substitution of one activity for another, satiation, level of aspiration, degrees of reality, reward and punishment, backsliding—what meaty, what human topics these are! There is a woeful gap in the psychology of motivation which may eventually be bridged by the work of these topologists and of the experimental psychotherapists whom I have previously mentioned. It is that *terra incognita* lying between the fairly well-mapped province of energetics (animal drives and endocrinology) and the somewhat less well-explored province of established adult interests (as surveyed by the methods of impression and expression, by word association, by attitude scales and interest blanks).

7. I must not omit—yet must not, for time's sake, elaborate—the wide variety of experimental investigations being made on abnormal persons in many hospital laboratories, represented for instance by some papers on our Wednesday afternoon program. There the clinical attitude is set aside, and the patients are studied not as individuals but as representatives of their disease classifications, and their various abilities and performances are investigated to furnish bases for generalization regarding, say, the effect of reduced insight upon the GSR, or the effect of color on neurasthenics, or the reaction time of manic-depressives, or the startle response in catatonics (**27, 28**).

These studies are in the great tradition of Kraepelin's laboratory

in those last decades of the Nineteenth Century in Heidelberg. For Kraepelin and Aschaffenburg, Sommer and Ranschburg did not restrict themselves to the viewpoint of a psychiatric examiner and therapist but sought out uniformities and general laws in the behavior of abnormal people—and meanwhile even devised many a piece of mechanical equipment now standard in our laboratories (21, 47).

8. For an eighth and final illustration let me point out the youngest and lustiest member of the whole psychological family: Child Development. The several very active child research centers in America are rather remarkable institutions. There come together brass-instrument-trained psychologists, personologists, pediatricians, physiologists, roentgenologists, dietitians, sociologists, and statisticians. In spite of their frequent adoption of the title "clinics," these centers are active in standardizing observational methods and in building up a sound experimental science of how the typical child develops. "A little child shall lead them"; and we are certainly learning that to be understood he must be studied from all sides and by coöperating specialists.

I would have failed to make my point if I had given the impression that this rapprochement between experimentally-inclined and clinically-inclined is only a professional or a personal one. It is a broadening of attitude on the part of each toward the *phenomena* of psychology. The older copper-riveted experimental viewpoint that human nature is something to be subjected to more and more isolated treatment away from the contaminations of the outside air has been giving ground. The human specimens set up in the psychological laboratory, like planets in their perturbations, have betrayed the influences of remoter forces. We can see this in the Gestalt psychology of perception (led by Wertheimer, Koffka, Köhler). We see it in the organismic psychology of reaction (Coghill, Carmichael, Wheeler—with apologies to each for this bracketing!), and in the plasticity of neural functioning (Franz, Lashley, Jacobsen, *et al.*). We see it in the reviving of maturational interpretations of child behavior (Gesell, Shirley, Pratt, McGraw, *et al.*—again with apologies for the grouping!), and in the dynamic nature of learning and forgetting (Bartlett, Maier, Lewin, *et al.*).

Meanwhile, the clinician, too, has been realizing some things about the phenomena of psychology with which he has dealt. The very stuff and subject matter which he has been handling called for more definiteness of treatment. Too often "the diagnosis was successful but the patient failed." Too often the prediction as to one's

best choice of vocation, or as to how to be happy though married, did not pan out. Most importantly, too often there were no precise checks wherewith to verify the sizing up and advice given to unhappy spouse or misfit employee or neurotic patient or parent of problem child. The human cases cried out for more certainty in the way they were to be analyzed, interpreted, advised. And so—with what must have been a touch of nostalgia—the practitioner has been harking back to the laboratory, and has been hoping that many of his inferences and his explanatory conceptions might be subjected to the *experimentum crucis.*

VIII.

And now to conclude. Tonight I have rejected the more colorful mantle of the destructive or constructive critic to don the historian's. Instead of exhibiting blueprints of one more system to end all systems I have assumed the rôle of narrator. I have tried to lift our view above the trees that we may plot the woods.

The sessions of individual papers that furnish the main substance of an annual meeting of our Association represent for the most part the blazing of new trails into the unknown in diverging directions. They are the reports of specialization. And if taken alone they might lend some basis for the acrid outside criticism that psychology is a medley of discordant themes. Surely it is fitting, then, that for one evening we might give our attention to harmonizing trends, to some of those ways in which the very subject matter of psychological research is leading to more mutual understanding and coöperative effort.

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FORMS OF CITATION ADOPTED BY THE BOARD OF EDITORS OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

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At the meeting of the Board of Editors of publications of the American Psychological Association in Chapel Hill, North Carolina, on April 6, 1938, it was voted to adopt for the journals of the American Psychological Association the form of bibliographical citation used in the *World List of Scientific Periodicals, 1900–1933,* and now employed by *Psychological Abstracts.* This will have the advantage of making our usage conform to the widely used standard of the "World List." Abbreviations of journals not in the "World List" are based on the abbreviations of the same words in other titles which do appear in the "List" and the practice which is clearly followed by the "World List."

The writer was asked to collect for publication a list of the abbreviations most likely to be used in these journals. Professor Walter S. Hunter has been good enough to supply me with those most commonly appearing in the *Abstracts.* The list which follows includes these, and since the abbreviations are readily interpretable without the full titles, the latter have not been added.

A short summary of the methods used in the derivation of these abbreviations of journal titles follows:

1. The initial letters of nouns are capitalized, while those of adjectives are not, except in the case of English adjectives formed from proper names.
2. Articles and prepositions are, as a rule, deleted, as is *and* in all languages, except when the latter occurs in titles containing only 2 nouns connected by *and,* and when it appears between broken compounds.
3. Words appearing at or near the beginning of a title are not abbreviated to the extent that those appearing later may be.
4. Singulars and plurals are not differentiated, and allied words (the the same words used in different languages, as Academy, Académie, etc.) retain the same form.
5. When words have been contracted, the full stop is omitted, whereas in the case of abbreviations it is employed.

6. The place of publication is necessary only when 2 or more journals bear the same title or when the abbreviated form would confuse the reader as to the nationality of the journal.
7. With a few exceptions, Japanese and Russian titles are transliterated (although this is not the practice when there is a subtitle in a European language, in which case the subtitle is abbreviated and is followed by the place of publication, which is set off by a comma).
8. In the case of long compound adjectives in Germanic and Scandinavian languages, a contraction is made and a hyphen is used to separate the parts (agrikulturekonomisch=agrik.-ekon.). When compound nouns occur they are abbreviated by parts as if these parts were separate (Entwicklungsmechanik=EntwMech.).

It is to be the practice, as with *Psychological Abstracts*, to capitalize only the initial letter of the first word of a title of an article except in the case of proper names. When there are 2 or more authors, the conjunction is indicated by the sign & rather than by *and*. The following title is an example:

> Shakow, D., & Harris, A. J. Scatter on the Stanford-Binet in schizophrenic, normal, and delinquent adults. *J. abnorm. (soc.) Psychol.*, 1938, 33, 100–111.

The form of citation for books is sufficiently indicated by the following titles:

> Crafts, L. W., Schneirla, T. C., Robinson, E. E., & Gilbert, R. W. Recent experiments in psychology. New York: McGraw-Hill, 1938.
>
> Jacobson, E. Progressive relaxation. (2nd ed.) Chicago: Univ. Chicago Press, 1938.
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> Lorge, I. Psychological problems in radio. *In* Marsh, C. S., *Educational Broadcasting 1936*. Chicago: Univ. Chicago Press, 1937. Pp. 231–234.
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> Müller, G. E. Abriss der Psychologie. Göttingen: Vandenhoeck & Ruprecht, 1924.
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> Whitehead, T. N. The industrial worker. (2 vols.) Cambridge: Harvard Univ. Press, 1938.

When only one volume of a book of more than one volume is cited, the volume number is placed after the date of publication, *i.e.* New York: Macmillan, 1938. Vol. I.

Finally, when an unpublished thesis is referred to, it may be cited as follows:

> Title. Unpublished Master's Thesis, Univ. Iowa, 1930.

It is urged that authors who submit manuscripts to any of the journals of the American Psychological Association hereafter follow this usage.

Acta biotheor.
Acta ophthal., Kbh.
Acta oto-laryng., Stockh.
Acta psychiat., Kbh.
Acta psychol., Hague
Acta psychol. Keijo
Allg. Z. Psychiat.
Amer. Ann. Deaf
Amer. Anthrop.
Amer. Ass. Adult Educ. Adjustm. Serv. Rep.
Amer. J. Ophthal.
Amer. J. Optom.
Amer. J. Orthopsychiat.
Amer. J. phys. Anthrop.
Amer. J. Physiol.
Amer. J. Psychiat.
Amer. J. Psychol.
Amer. J. Sociol.
Amer. Nat.
Amer. sociol. Rev.
An. Inst. Psicol. Univ. B. Aires
Ann. Eugen., Camb.
Ann. math. Statist.
Ann. méd.-psychol.
Ann. Otol., etc., St Louis
Année psychol.
Arbeitsphysiologie
Arch. Anthrop. crim.
Arch. argent. Psicol. norm. pat.
Arch. Augenheilk.
Arch. gen. Neurol. Psichiat.
Arch. ges. Psychol.
Arch. ital. Biol.
Arch. ital. Psicol.
Arch. KrimAnthrop.
Arch. néerl. Physiol.
Arch. Neurobiol.
Arch. Neurol. Psychiat., Chicago
Arch. Ohr.-, Nas.-, u. KehlkHeilk.
Arch. Ophthal., N. Y.
Arch. Psychiat. Nervenkr.
Arch. Psychol., Genève
Arch. Psychol., N. Y.
Arch. Rass.- u. GesBiol.
Arch. Speech
Arkh. biol. Nauk
Atti Soc. ital. Progr. Sci.
Aust. Coun. educ. Res. Ser.
Aust. J. Psychol. Phil.

Beih. Z. angew. Psychol.
Beih. Zbl. Psychother.
Biol. Abstr.
Biol. Bull. Wood's Hole
Biol. Rev.
Biol. Zbl.
Biometrika
Biotypologie
Brain
Brit. J. educ. Psychol.
Brit. J. med. Psychol.
Brit. J. Ophthal.
Brit. J. physiol. Optics
Brit. J. Psychol.
Brit. J. Psychol. Monogr. Suppl.
Bull. Acad. Belg. Cl. Sci.
Bull. Acad. Sci. U. R. S. S.
Bull. (Ann.) Soc. ent. Belg.
Bull. biol.
Bull. Biol. Méd. exp., U. R. S. S.
Bull. Menninger Clin.
Bull. neurol. Inst. N. Y.
Bull. Off. intercommun. Orient. prof. Brux.
Bull. Purdue Univ.
Bull. Sch. Educ. Ind. Univ.
Bull. Soc. Sci. Liége
Bur. Stand. J. Res., Wash.

Cath. educ. Rev.
Cath. Univ. Amer. educ. Res. Monogr.
Cervello
Character & Pers.
Child Develpm.
Child Develpm. Abstr.
Child Develpm. Monogr.
Child Res. Clin. Ser.
Chin. J. Psychol.
Cold Spr. Harb. Sympos. quant. Biol.

Comp. Psychol. Monogr.
Contr. psychol. Theor.
C. R. Acad. Sci., Paris
C. R. Acad. Sci. U. R. S. S.
C. R. Soc. Biol. Paris
Cult. & Educ.

Dtsch. Z. Nervenheilk.

Educ. Trends
Elem. Sch. J.
Encéphale
Endocrinology
Erkenntnis
Eugen. News
Eugen. Rev.
Évolut. psychiat.

Fact. industr. Mgmt
Forsch. Fortschr. dtsch. Wiss.
Fortschr. Neur. Psychiat.

Genet. Psychol. Monogr.
v. Graefes Arch. Ophthal.
Growth

Handb. biol. ArbMeth.
Harv. educ. Rev.
Hospitalstidende
Hum. Biol.
Hum. Factor, Lond.

Imago, Lpz.
Indian J. Psychol.
Industr. Psychol.
Industr. Psychotech.
Int. J. indiv. Psychol.
Int. J. Psycho-Anal.
Int. Z. Indiv.-Psychol.
Int. Z. Psychoanal.

Jap. J. appl. Psychol.
Jap. J. exp. Psychol.
Jap. J. Psychol.
Jb. Kinderheilk.
Johns Hopk. Hosp. Bull.
J. abnorm. (soc.) Psychol.
J. acoust. Soc. Amer.
J. Amer. med. Ass.
J. Amer. statist. Ass.
J. appl. Psychol.
J. belge Neurol. Psychiat.
J Biol. Méd. exp., Moscou
J. cell. comp. Physiol.
J. comp. Neurol.
J. comp. Psychol.
J. consult. Psychol.
J. crim. Law Criminol.
J. educ. Psychol.
J. educ. Res.
J. educ. Sociol.
J. except. Child.
J. exp. Biol.
J. exp. Educ.
J. exp. Psychol.
J. gen. Physiol.
J. gen. Psychol.
J. genet. Psychol.
J. Hered.
J. higher Educ.
J. industr. Hyg.
J. juv. Res.
J. ment. Sci.
J. Negro Educ.
J. nerv. ment. Dis.
J. Neurol. Psychopath.
J. Neurophysiol.
J. opt. Soc. Amer.
J. Organother.
J. Parapsychol.
J. Phil. (formerly J. Phil. Psychol. sci. Meth.)
J. Physiol.
J. Psychol.
J. Psychol., Moscou
J. Psychol. Neurol., Lpz.
J. Psychol. norm. path.
J. soc. Phil.
J. soc. Psychol.
J. Speech Disorders

Kentucky Person. Bull.
Klin. Wschr.
Kwart. psychol.

Lancet

Med. Rec., N. Y.
Mém. phys.-mat. Acad. Sci. Ukr.
Ment. Hyg., Lond.
Ment. Hyg., N. Y.
Metron

Mind
Monogr. Soc. Res. Child Develpm.
Mschr. Kinderheilk.
Mschr. KrimBiol.
Mschr. Psychiat. Neurol.

Nature, Lond.
Nav. med. Bull., Wash.
Ned. Tijdschr. Psychol.
Nerv. ment. Dis. Monogr. Ser.
Neue psychol. Stud.
New Scholast.

Occupations
Occup. Psychol.
Occup. Ther.

Peabody Coll. Contr. Educ.
Peabody J. Educ.
Person. J.
Pflüg. Arch. ges. Physiol.
Phil. Rev., N. Y.
Phil. Sci.
Philos. Trans.
Physiol. Rev.
Physiol. Zoöl.
Polsk. Arch. Psychol.
Pr. méd.
Proc. Amer. Ass. Stud. ment. Def.
Res. Publ. Ass. nerv. ment. Dis.
Proc. nat. Acad. Sci., Wash.
Proc. roy. Soc.
Proc. roy. Soc. Edinb.
Proc. Soc. exp. Biol., N. Y.
Proc. Soc. psych. Res., Lond.
Psyche, Lond.
Psychiat. neurol. Bl., Amst.
Psychiat. Quart.
Psychiatry
Psychoanal. Quart.
Psychoanal. Rev.
Psychol. Abstr.
Psychol. Bull.
Psychol. Clin.
Psychol. Exch.
Psychol. Forsch.
Psychol. Index
Psychol. Monogr.
Psychol. Rec.
Psychol. Rev.
Psychometrika
Psychometr. Monogr.
Publ. Hlth Bull., Wash.
Publ. Hlth Rep., Wash.
Publ. Opin. Quart.

Quart. J. exp. Physiol.
Quart. J. Speech
Quart. Rev. Biol.

Rass. Studi psichiat.
Relig. Educ.
Rep. industr. Hlth Res. Bd, Lond.
Rev. franç. Psychanal.
Rev. gén. Sci. pur. appl.
Rev. neurol.
Rev. Neurol. Psychiat., Praha
Rev. Phil., Paris
Rev. Psicol. Pedag.
Rev. sci., Paris
Riv. Patol. nerv. ment.
Riv. Psicol. norm. pat.
Riv. sper. Freniat.

Sch. Rev.
Sch. & Soc.
Schweiz. Arch. Neurol. Psychiat.
Schweiz. med. Wschr.
Science
Scientia, Milano
Sci. Mon., N. Y.
Skand. Arch. Physiol.
Smith Coll. Stud. soc. Work
Sociometry
Spec. Rep. Ser. med. Res. Coun., Lond.
Stud. Psychol. Psychiat. Cathol. Univ. Amer.
Svenska Läkartidn.
Svenska LäkSällsk. Handl.

Teach. Coll. Contr. Educ.
Teach. Coll. Rec.
Teach. Forum (Blind)
Tech. Rep. Hum. Engng Lab.
Theoria
Tidsskr. norske Laegeforen.
Tohoku psychol. Folia
Train. Sch. Bull.
Travail hum.

Umschau
Univ. Calif. Publ. Psychol.
Univ. Ia Exten. Bull.
Univ. Ia hum. Stud.
Univ. Ia Stud. Aims Progr. Res.
Univ. Ia Stud. Charact.
Univ. Ia Stud. Child Welf.
Univ. Ia Stud. Educ.
Univ. Ia Stud. Psychol.
Univ. Ia Stud. Psychol. Music
Univ. Ia Stud. soc. Sci.
Untersuch. Psychol. Phil.
U. S. Child. Bur. Publ.
U. S. Off. Educ. Bull.
U. S. Veterans' Bur. med. Bull.

Valsalva

Yearb. nat. Soc. Stud. Educ.

Z. angew. Psychol.
Z. Arbeitspsychol.
Z. Biol.
Z. ges. Neurol. Psychiat.
Z. Kinderforsch.
Z. Kinderheilk.
Z. Kinderpsychiat.
Z. Morph. Anthr.
Z. pädag. Psychol.
Z. Parapsychol.
Z. psychoanal. Pädag.
Z. Psychol.
Z. Psychother. med. Psychol.
Z. Sinnesphysiol.
Z. Tierpsychol.
Z. vergl. Physiol.
Zbl. Psychother.
Zh. Nevropat.
Zool. Jb.

A NOTE ON FOLEY'S REVIEW OF THE LITERATURE ON 'MOTOR SPEED AND TEMPO'

BY LYLE H. LANIER

Vassar College

In the course of his recent review of the work on 'motor speed and tempo,' J. P. Foley commented as follows upon an article by Lambeth and the writer:

"Lambeth and Lanier have attempted further to analyze experimentally Klineberg's view that racial differences found on many psychological tests are essentially differences of speed. The Stanford-Binet scale, a rational learning test and six tests of rate of response were given to 30 white and to 30 Negro twelve-year-old boys. Comparisons showed that the more complex the test, the higher the relative score made by the whites; and since there was roughly a parallel increase of speed difference in their favor, Lambeth and Lanier maintain that the theory of a general speed difference between the races is untenable. Since the intercorrelations between the various tests were found to be higher for the Negroes, they conclude that this is possibly indicative of a 'more primitive' and 'less differentiated' type of mental organization. For a critical evaluation of such antiquated, mentalistic views the reader is referred to Boas (**10**), Klineberg (**57**) and Radin (**88**)" (p. 362).[1]

In the first place, Foley misrepresents the position taken by Miss Lambeth and me with respect to the intercorrelations reported. The following paragraph from our article should clarify this point:

"The fact that the average *amount* of intercorrelation is much higher in the case of the Negroes seems to argue for the relatively greater operation of a 'general factor' among the conditions which determine Negro performance. Undoubtedly, the low numerical magnitude of many of the individual correlation coefficients and their statistical unreliability, in many instances, make it impossible to draw any final conclusions. But the results are sufficiently definite in the several sets of correlations (especially in the case of well-selected subjects and carefully administered tests) to define this as an important problem for further research. This higher intercorrelation might conceivably be due to any one of several causal factors or to a complex of factors. We have already mentioned, and dismissed as far as these data go, the possibility that greater heterogeneity in the Negro sampling might operate to produce these higher correlations. It has been suggested that the presumably more differentiated environment of the white might produce a greater specialization of 'abilities' which would result in a lower average intercorrelation among test scores for that race. Or the Negro might be basically of a simpler, more homogeneous, more 'primitive' type of organization. *We cannot conclude from*

[1] Foley, J. P. Factors conditioning motor speed and tempo. *Psychol. Bull.*, 1937, **34**, 351–397.

our data which hypothesis, if either, is correct and speculation is useless. The problem must be left, as indicated above, to future research" (p. 278; italics mine).[2]

In the second place, although we definitely did not adopt the hypothesis imputed to us by Foley, such a view would be neither "antiquated" nor "mentalistic." The various conceptions of "mental organization" find their definitions in terms of the operations involved in mental testing and in factor analysis. It is conceivable that these procedures will become antiquated, but it would be very discouraging to many of our psychologists to offer such a prognosis now! In any case, the current concept, "mental organization," carries no "mentalistic" connotations in the dualistic sense.

[2] Lambeth, M., & Lanier, L. H. Race differences in speed of reaction. *J. genet. Psychol.*, 1933, **42**, 255–297.

SPECIAL ANNOUNCEMENT

The department of psychology of the Johns Hopkins University announces a reorganization which has recently gone into effect. This reorganization aims to provide for psychology an unusual amount of coöperation from other departments. In fact it may be said that the new department is to a large extent a focus of interest in which psychological aspects of the activities of other departments are collected and applied to the training of psychology students.

Two types of coöperation are available. There are, of course, regular exercises in philosophy, statistics, genetics, physiology, neurology, psychiatry, and mental testing given by other departments. In addition to these regular features there are various original investigations in other departments which have important bearings on psychological problems. Among the topics which other departments at Johns Hopkins frequently or regularly investigate, one might mention animal behavior (by Mast in zoology and Richter in psychiatry), conditioned reflexes (by Gantt and associates in psychiatry), emotion and neural mechanisms (by Bard in physiology), physiological optics and vision (by Sloan and associates in the Wilmer Institute), and personality (by Meyer and associates in psychiatry).

To those concerned with the reorganization of the department it seemed that any rational plan for psychology must comprehend these valuable but scattered resources. Moreover, it seemed that an institution like Johns Hopkins, where departmental boundaries may be crossed so freely, would be a good place for an experiment demanding so much departmental coöperation. All in all it seemed that the University might do more for psychology by concentrating these various agencies on a limited number of psychological problems than it could by merely duplicating features of good departments in other universities.

It will be the function of the department of psychology to provide the essential training in psychology and to help students to integrate the training received in other departments and to apply it to the problems of psychology. While the department will offer a few basic courses in psychology, it is hoped that for the most part these functions may be fulfilled through informal individual work.

Since the department is organized around coöperation, its program of graduate training will be limited to those aspects of psychology, such as physiological and general psychology, for which necessary collaboration is available. There will be no attempt, for instance, to train specialists in social psychology since there are no departments to provide the necessary training in cultural anthropology, sociology, and geography. At present, the support from psychiatry will be utilized for general training only. The department of psychology will not attempt to train students who wish to specialize in abnormal psychology for clinical purposes.

Some details may help to clarify the plan. Only 4 students will be accepted each year. Training will consist largely of directed individual work in library and laboratory. The first half year will be spent almost entirely in psychology. The second half year and a portion of the second year will be taken up with courses in other disciplines. The entire program for the Ph.D. will require somewhat more than 3 years. For students admitted at the end of the junior year in college, the minimum residence is 4 years. The undergraduate program will not be greatly affected by these changes.

No changes in the plans nor additions to the department are contemplated until there has been an opportunity to determine how the present program functions.

Dr. J. M. Stephens and Dr. S. M. Newhall are in charge of the arrangements and are assisted by a committee of instructors from other departments under the chairmanship of Dr. Philip Bard.

BOOK REVIEWS

STEVENS, S. S., & DAVIS, H. Hearing: its psychology and physiology. New York: Wiley, 1938. Pp. xv+489.

As is brought out in the preface, the task assumed by the authors of this book was twofold. They sought to produce an inventory of recent investigations of hearing, and also to discover for themselves whether a consistent point of view is possible in this field.

The book deals first with the phenomena of hearing, and then proceeds to a discussion of the anatomy and physiology of the ear. The style of treatment is direct and objective. A great amount of material has been covered and is presented in a well-organized form and with commendable accuracy.

The authors have intentionally limited their treatment in certain respects. The historical development of the various problems has been left out of account in all but a few instances, and for the most part no attempt has been made to indicate the original sources of material and ideas. Therefore, the student who is not already oriented in the field may not be able fully to appreciate many of the recent developments in relation to the work of the past. For example, the description of the manner of movement on the basilar membrane in response to sound (p. 343) is that of Ter Kuile, yet no reference is given. Again, the discussion of the degeneration of auditory fibers after intracranial section of the nerve (p. 339) includes a reference to Crowe, who adduced evidence in support of Wittmaack's original observations, and none to Wittmaack himself. The reader therefore must be on his guard against misconception on the matter of authorship and must take the book for no more than the writers intend: a portrayal of the present status of auditory problems without serious regard for historical developments.

In the preface the authors refer to the absence of general theory, and regard it as a consequence of the principle that "theories flourish on a certain sparseness of facts and wither in the face of abundance." They allow themselves a liberal amount of speculation in the interpretation of specific phenomena, but attempt no general theoretical formulation. However, a place theory of hearing is implicit throughout the treatment. An appendix (pp. 444 ff.) refers to the contributions of Reboul to a nonresonance place theory.

The reviewer agrees that when once we know enough about the ear and its actions we shall have no need of theories, but he is unable to find that the time is yet come. The authors' speculation on specific phenomena is a clear indication that our knowledge is incomplete. The danger of this type of speculation, in the absence of general theoretical considerations, is that at different times one may make assumptions that are unreconciled. An example may make this point clear. The authors (pp.

97 ff. and elsewhere) relate pitch and pitch discrimination to the assumption that "to each audible frequency there corresponds a position on the basilar membrane 'tuned' to that frequency." It is made plain that on this assumption pitch discrimination is a function of the degree of separation of two stimulated areas of the membrane, and, thus, that the area for a single tone must have a certain degree of discreteness and isolation. On the other hand, the authors mention, in the discussion of stimulation deafness and related pathological conditions (p. 372), that high intensities of stimulation for single tones produce extensive spread of excitation. The theoretical difficulties become apparent when we consider (see pp. 88 ff.) that at high intensities of stimulation pitch discrimination is not worse but actually better than near threshold.

A closer consideration of the content of the book will follow the order of its chapters. The book is divided into two parts, which correspond to the individual interests of the two authors. The first part deals with psychological phenomena, while the second part is concerned mainly with anatomy and physiology.

The first chapter treats briefly the nature of auditory stimuli and means for their production. The next eight chapters on auditory phenomena form what is plainly the most valuable section of the book. The topics treated are sensitivity, pitch, loudness, volume, density, localization, harmonics, combination tones, masking, fatigue, persistence, and modulation. The plan of treatment is one in which particular papers in the field of each topic are reported in considerable detail, without much comment or criticism. The result is a picture of the present status of auditory problems and research, but naturally a picture that is often incomplete. The choice of papers for consideration may at times seem arbitrary, but for the most part can easily be justified as the most recent or the most general in treatment of the problem. The reader is thus given a series of intimate views within the field, with bibliographic material which, though limited, will in most instances enable him to explore further for himself. The treatment is more complete for some of the topics, to which the authors themselves have made experimental contributions.

Particularly useful is the account in Chapters 3, 4, and 9 of the work on transient sounds, especially the studies of Steudel and of Bürck, Kotowski, and Lichte, which are somewhat difficult of access for many students.

Issue may be taken with the authors on certain technical points, such as the following:

The curve given for the threshold of 'feeling' (Fig. 20, p. 61), represents old data (and a good deal of guesswork); better results for this function are available, and had already been given in Fig. 19.

The use of 'percent hearing loss' as a way of indicating degrees of auditory impairment is accepted, though the authors point out certain mathematical disadvantages (pp. 61 ff.). However, the 'percent hearing loss' designation has the further fundamental defect that it does not discriminate between regions of frequency that are important and those that are relatively unimportant for the appreciation of speech and music,

and thus bears no close relation to the practical aspects of deafness. Therefore, in the reviewer's opinion, this designation should be discarded.

It is stated (p. 64) that in testing the hearing of a normal person by bone conduction "the external ear canal must be stopped off." This procedure is undesirable in that it alters the aculty of the ear.

Data on the relation of sensitivity to age are given (pp. 67 ff.) from the 1929 study by Bunch. More extensive data are available in the work by Bunch and Raiford in 1931.

An attempt is made (p. 72) to relate the results on pitch variations with intensity to the equal-loudness contours. After pointing out that the minima of the equal-loudness contours (Fig. 45, p. 124) represent the points of maximal sensitivity at different loudness levels, the writers state that "there is clearly a slight shift of the minima in the direction of the higher frequencies as the intensity is increased." An examination of the figure referred to shows that as the intensity is increased the minima shift toward the lower frequencies from threshold to the 40 or 50 db. level; and then at higher levels of intensity there appear two minima, one that shifts toward the lower and another that shifts toward the higher tones.

The writers seek to explain the variations of pitch with intensity by the actions of the tensor tympani muscle. Contraction of this muscle is said "to impede the transmission of low tones," and thus to account for the shift in the minima of the pitch contours (Fig. 23). In regard to the effects of tension of the intra-aural muscles on transmission in guinea pigs, the authors state (p. 266) that low tones are reduced as much as 45 db., tones around 1000~ are unchanged, those around 1300~ are slightly improved, and those above 2000~ are unchanged. At another place (p. 322) it is said that the reduction of transmission by tension of the intra-aural muscles is "absent for frequencies above 2000 cycles," and, further on (p. 351), that "at frequencies above 1000 cycles . . . the transmission of sound is not affected by this reflex." The authors do not explain why, if tensor contraction is ineffective for high tones, they nevertheless obtain very different curves for pitch as a function of intensity for tones from 2000 to 12,000~. It should be said further that experiments in other laboratories show effects of tensor contraction, in guinea pigs and cats, for high tones as well as low.

Results obtained by other investigators on the relation of the tensor tympani muscle to aural distortion do not agree with those mentioned (pp. 190 ff.).

In the second part of the book, three chapters are devoted to anatomy and physiology, largely at an elementary level.

It is said (p. 271) that "the under surface of the basilar membrane is covered by vascular connective tissue." This is a mistake; the tissue contains only one vessel, and thus can hardly be termed 'vascular.'

The statement (p. 273) that the basilar membrane "becomes progressively and systematically wider toward the helicotrema" is not in agreement with the results of Guild (1927) on the guinea pig or with recent data on man. Guild observed that the width of the basilar membrane varies greatly in its rate of increase from the basal end upward. There

are considerable individual variations, but usually the membrane attains its maximum width a few mm. from the apex and then rapidly grows narrower. Similar relations hold for man. This criticism applies also to the diagram of Fig. 140 (p. 370), which represents the basilar membranes of guinea pig and man as uniformly tapered. (Incidentally, in Fig. 114, the round window is labeled 'memb. tot.'; cf. Fletcher, Wrightson, and Keith.)

The final six chapters are mainly concerned with the electrical responses of the cochlea and auditory nerve. The material is largely derived from investigations in the Harvard laboratory by the authors and their collaborators. This section of the book, therefore, is valuable particularly as a presentation of the writers' latest views in this field. An excellent discussion of the origin and nature of cochlear potentials is included.

The writers have clarified their previous descriptions of the form of the intensity function for the cochlear response. They have also clarified their earlier use of the term 'thresholds' as applied to this response, and agree that what appears to be a threshold is an instrumental artifact. However, they continue their use of this 'threshold' as though it were valid.

The authors cite results on the effects of local cochlear damage on cochlear responses (pp. 366 ff.), and reproduce the curve in which they represent a correlation between location of damage and impairment of response. The data reproduced here (Fig. 138), like those previously given, seem much too irregular for the precise interpretation given, and insufficient emphasis is placed upon the extensive character of the losses. There are many matters of this kind that need to be enlarged by a consideration of the work of other investigators, as well as by further experimentation.

The book is unusually well made from a technical standpoint. The printing is clear and remarkably free of typographical defects. The illustrations are abundant, well chosen, and skillfully reproduced. A few minor errors occur in the list of references.

This book stands somewhere between an elementary treatise and a handbook: it is too sophisticated for the former, but not complete enough for the latter. But despite the limitations of treatment, which we can charge to the inevitable limitations of space, the book is a distinct contribution to the field of hearing. It will be welcomed both by the beginning student as an introduction to the field, and by the advanced student as a convenient source of material.

E. G. Wever.

Princeton University.

Barcroft, J. The brain and its environment. New Haven: Yale Univ. Press, 1938. Pp. vii+117.

Sir Joseph Barcroft, for many years professor of physiology at Cambridge University, has presented in this brief volume of Terry

Lectures a suggestive discussion of some of his recent work. In the preface he writes:

"I shall attempt to discuss in the last chapter, and lead up to in those which precede it, certain phases of what the individual *does*, and certain factors which influence not only his actions but the mental processes which lie behind these actions.

"In the first chapter, therefore, I will say something about the very origins of behavior in the embryo and the influence on these primitive happenings of some crude alterations in the foetal environment. In the second, I treat of the transition from the foetal to the postnatal condition. How does the alteration in the environment affect the organism? And so to my point of view with regard to the environmental conditions under which the mind has reached its present development."

After a discussion of relations between the maternal and foetal circulation, which latter constitutes the internal environment of the embryo, the first chapter deals with brain activity in mid-foetal life, as exemplified by studies of behavior of the sheep foetus at varying age levels in response to mechanical stimulation. A rather definite pattern of ordered responses to the same stimulus is observable:

"The general picture presented by the central nervous system is that of a mechanism which develops the power of jerky movement early. Later, these jerky movements become more drawn out and irradiation more limited, and later still inhibition becomes developed and obscures movement. In the reverse order, as these traits are removed by interference with the normal environment, [as by asphyxia] the foetus simulates in turn ones in which the brain has been severed at progressively descending levels. Starting with a condition of quiescence, it passes through a stage of sustained extension to one of jerky movement and finally it becomes immobile and dies. So also as the foetus deteriorates, it progressively reverts to the properties of earlier days."

* * *

The second chapter deals with brain activity shortly after birth from the point of view of (1) the exposure of the skin and receptors to the atmosphere and of the body to gravitational conditions and (2) the aeration of the blood. It is the effect of changes in this latter, readily quantifiable variable on the action of the respiratory and cardiac centers which is the author's chief concern. This chapter does not lend itself to a summary statement.

The last lecture deals with "mental efficiency considered in relation to some properties of the blood." Starting with the famous dictum of Claude Bernard: "The constancy of the *milieu intérieur* is the condition of a free life," the author considers what is meant by freedom of behavior and how fluctuations of the *milieu intérieur*, such as changes in temperature, O_2, glucose, water, cH, Na, and Ca of the blood, modify it. In concluding this chapter, he writes:

"We have therefore arrived at the point where we can say not only that the form of life—man—which has developed an intellectual power far beyond any other, is that in which the constitution of the fluids that bathe his cells is most exactly regulated; but also that the most immediate effect of interference

with the chemical or physical properties of the blood is impairment of the higher qualities of the brain."

The monograph is essentially a suggestive introduction to a very large problem. As the author points out, the data are often meager and the conclusions necessarily tentative. More questions are raised than are answered, but this is inevitable and, if taken in what this reviewer believes to be the spirit of the author, does not detract from the monograph.

To many American psychologists the rather cavalier use of mentalistic and physiological terminology, especially characteristic of British physiologists when they write about behavior, may detract from the value of the contribution. However, to some this will be a matter of small moment in view of the nature of the problems dealt with and the promise of the approach.

HUDSON HOAGLAND.

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ELLIS, W. D. A source book of Gestalt psychology (with an introduction by K. Koffka). New York: Harcourt, Brace, 1938. Pp. xiv+403.

This book boils down into 400 pages of English as against over 2000 pages of original German text. The attempt is made to present the most important original works of the Gestalt school between 1915 and 1929, but the temporal limitations are by no means arbitrary. The work before 1915 is either pretty well known to American psychologists (for example, Wertheimer on movement), or is not yet full-blown Gestalt theory (for instance, Koehler on sound). By 1929 some of the leaders of the school had already taken up permanent residence abroad and others were beginning to "commute" back and forth, so that after 1929 much of the important Gestalt literature was being published in English and the then current German literature was being exposed in English. It is also worth noting that in 1929 the Great Depression was already under way and Hitler was ceasing to be a joke and becoming a menace.

Nor is the choice of selections arbitrary. Dr. Ellis has attempted to make available to the English-speaking student works previously available only in German. The work is honestly planned to be of service to the serious student of psychology. Consequently, neither the English writings of the Gestalt-theorists nor those of their works which had been adequately translated are included. This means, of course, that the "Source Book" is not representative in the sense of being a "gems-of-Gestalt-theory" type of anthology. Including nothing from Koehler's *Gestalt psychology* or *Mentality of apes* or from Koffka's *Growth of the mind* and very little from Lewin, the book cannot be looked on as completely "representative." However, the serious student of psychology already knows these things, so any lack of "representativeness" which the book may have as an anthology is more than made up for by the inclusion of so much material not heretofore translated. (The only overlaps are the inclusion in abstract form of eighteen pages of Lewin's *Will and needs*, which has already been published in English in his *Dynamic*

theory of personality, and v. Hornbostel's *Unity of the senses*, which had been previously translated. These are minor duplications in a work of this magnitude.)

The book includes four works on general theory, fourteen on perception, four on animal psychology, two on thought, two on psychical forces, six on pathological phenomena, and three polemical replies to critics. I have run no definite statistic on the fairness of this selection with regard to the total published literature. I have, however, paged through the *Psychologische Forschung* (where over half of the contributions were first published) for the years in question and the allotment of space to the various topics seems to be quite representative of the amount of work done in each field.

All compilers of anthologies must pick and choose. I would have excluded perhaps five of the papers here included and substituted others. Such differences of opinion are really matters of taste. Whether these papers represent the *most* important and those only does not have to be decided. Certainly differences of opinion on this aspect of the "Source Book" will be few and slight.

Dr. Ellis arrives at his condensations by a novel method which is partially abstraction and partially translation. Some papers are given practically in full, and others are greatly cut. I believe that the author has been very successful in his endeavors to give the gist of so many pages in so few. Some of the papers even gain in sharpness and clarity. German scientific writing always tends to be verbose from our standards. In condensing, naturally, many fine points and much illustrative material are excised. If one is saved considerable reading time by the manner of presentation, one must expect to spend much more intellectual effort to follow the argument in places. (In this connection I have reread the duplicated pages of Lewin's *Will and needs*, referred to above. One loses but little in Ellis' condensation compared with Adams and Zener's translation but considerably more effort is expended in following the condensation.) The pagination of the original text is printed throughout in the margins, however, so that, should the reader become lost, he can quickly find the pertinent passage in the original.

This book was five years in the making and obviously a great deal of painstaking scholarship has gone into it. Dr. Ellis had Professor Koffka's counsel in the choice of selections and in the condensation. With the publication of the book the last excuse is gone for not "understanding" Gestalt psychology. The profession should be grateful to Dr. Ellis for this labor of scholarly love.

So much for what the book purposes and how effectively it accomplishes its ends. I believe it is pertinent, now that we have so much of the essential Gestalt literature in English and between two covers, to ask: What is the Gestalt theory as a movement in modern psychology and what is its place in modern culture?

From the papers in this book it is easy to see what Gestalt psychology is *not*; and seeing what it is *not* is important because it has been accused of being all these things by some very eminent critics. In the first place, it is *not all* theory. The book abounds in intelligently planned and skill-

fully executed experiments. Many of the experiments are truly elegant and contemporary American psychology has few "great" experiments to compete with these. It is *not* a new system of high-sounding metaphysics. The idea of supersummative wholes, an idea which originally appeared neither grammatical nor mathematical to American experimentalists, is a very useful research tool. In recent times it has become possible to present the idea of supersummative wholes in terms of field theory and so remove every last vestige of metaphysical implication. It is *not* a sort of vitalism and hence "soft-boiled" compared with behaviorism. Every theoretical and polemical paper in this book contradicts this assumption. It is *not* simply a clever idea for explaining certain perceptual phenomena. That perception was first successfully studied by the Gestalt-theorists is true; but as this book gives abundant evidence, animal behavior, learning, psychopathological phenomena, and the dynamics of action were soon investigated by them. In most recent times (I judge by the recent issues of *Psychologische Forschung*) memory and emotion have even replaced perception as the chief field of endeavor.

What *is* Gestalt psychology then? In the light of the researches summarized in this book the Gestalt psychology of the twenties appears as the first conscious and integrated attempt to treat psychological problems in terms of an hypothetico-deductive method. The older established sciences, like physics, had been doing this for several centuries, but with academic psychology the theory either came as an afterthought (as in the neurological theories of the nineteenth-century workers in perception) or as something to start with and then forget about (as in the hands of the naïve behaviorists). The Gestalt idea was a good theoretical postulate from which certain deductions come quite easily. And the Gestalt-theorists from the first verified such deductions experimentally. The experimental verification quite often led to a refinement of theoretical postulation, from which more precise experimentally verifiable deductions could be made. Theory and experiment are integrated hand-in-glove fashion throughout these papers.

In retrospect one can also see why it was so hard for the Gestalt-theorists at first to gain a sympathetic hearing in America. One reason lies undoubtedly in the literary style and manner of presentation. Since William James, American psychological writing has been almost unbelievably bad in style. We have also developed a tradition of writing too tersely along standardized sequences of content: purpose, history of the problem, experimental method, results, conclusion, summary. After performing elegant experiments or having keen ideas the German tends to expose them in his own personal literary style and, I must add, at a greater length than they sometimes warrant. Many of the papers presented in this book (particularly some of Wertheimer's and Hornbostel's) do not read like our idea of "scientific" papers at all.

Another thing which characterized the Gestalt-theorists was a certain, and I think justifiable, disdain of the secondary paraphernalia of "science," such as unnecessarily elaborate apparatus, large statistical tables of questionable significance, time measurements to the sigma, and

space measurements to the millimeter when seconds and centimeters would prove the point. Many of the experiments—and they are experiments—reported here require neither apparatus, measurement, nor statistical treatment. But they do demonstrate quite adequately psychological facts which follow from Gestalt theory. In this volume are about fifteen percent of all the titles published in the *Psychologische Forschung* during the period. I very much doubt if any other psychological journal in the world could be combed to fifteen percent to produce a set of psychological papers of such excellence. Gestalt psychology is undoubtedly of great methodological significance.

Concerning its practical significance and its value to society I am much more skeptical. Both Koehler and Wertheimer make much of the nearness-to-life (Lebensnähe) of Gestalt psychology compared with the farness-from-life (Lebensferne) of other academic psychologies. The papers in this book remain very "academic" in both the best and the worst sense of the word. They represent the working out of the immediate scientific interests of a group of urbane, sensitive, intelligent, and at times genial men. These men took the continuance of the great German University tradition for granted. The problems which interested them had little or nothing to do with the sociopolitical crisis in Germany during the time. The index contains no reference to Hitler, National Socialism, war, or revolution. The Gestalt-theorists were working outside the "field" of their own culture. Here we find a clear-cut example of how academic psychology may fare in the modern world. The leaders of the most consequential of the modern German schools of psychology have had to leave the country.

Recently I had occasion to page through the *Zeitschrift für Psychologie* since 1933. If it would be hard to make a compilation of more excellent papers than that which Ellis has made from the *Forschung* (chiefly), it would be impossible to make a compilation of more nonsensical papers than could be made from the *Zeitschrift*. I could stomach but little of this rubbish, but I remember one long paper which demonstrated that the Gestalt-psychologists had "cooked" their results and another in which the characterological superiority of the Nordic chicken was exhibited. All this may or may not have something to do with the future of psychology as a science in these parts. I think that it does.

J. F. Brown.

Institute for Psychoanalysis, Chicago.

Stroud, J. B. Introduction to general psychology. New York: Prentice-Hall, 1938. Pp. xv+681.

Present-day writers of introductory texts in psychology must feel it incumbent upon themselves to decide whether the proposed book shall be one of an extremely popular or one of a more technical, less interesting sort. Some writers have built their texts around the presumed or tested interests of the prospective students; others avoid the issue by inserting some material solely for its presumed interest to the student; while others take the position that psychology is what psychologists have made it.

The book before us has taken the second of these three courses. To this end the author has included the photographs of twelve men who have figured prominently in the field of psychology: Boring, Thurstone, McDougall, Cannon, Herrick, Lashley, Pillsbury, Carr, Watson, Thorndike, Ebbinghaus, and Terman. Likewise he has included some material because of its interest without consideration of its systematic importance. But although "the question of the probable interest of the students has consistently been a determining factor in the selection or rejection of content material" (Preface, p. vii), the result is by no means an overpopularized text, and students will not find it an easy one. The author has not shirked the task of dealing with controversial topics which are scattered plentifully throughout the text material.

The chapter organization of the book differs markedly in some respects from other texts. Two chapters, the first on assumptions and methods, the second on some principles of behavior, open the book. Then follows a chapter on instinct and culture and a fourth on personality. This sequence is then sharply broken by a chapter on pleasantness and unpleasantness (somewhat of a departure as a chapter heading) and by a sixth on emotion. A chapter on the nervous system and another on nervous and motor phenomena make up the seventh and eighth chapters. Then follow chapters on sensory processes, the perceptual processes, and the symbolical processes. Chapters 12 and 13 are devoted to learning and to memory. Work, fatigue, and sleep make up the fourteenth chapter. A final chapter deals with intelligence. Perhaps there is no one way to cover the topics in psychology, and it is entirely possible that the particular order presented here lends itself more easily to teaching methods. Such an order, however, does not clearly reveal any systematic ordering of the field. Each chapter is followed by a series of questions for review and citations to pertinent experimental and theoretical papers.

The book "is not committed definitely to any systematic position," but "in the main, it follows behavioristic traditions in its fundamental concepts, although subjective material is included in the factual data" (Preface, p. vii). The behavioristic traditions followed are best seen in the statements that "mental activity is protoplasmic activity" (p. 8); that "*one form of protoplasmic change leads to another. This is the key to all behavior*" (p. 18; italics author's): "an idea is a protoplasmic activity" (p. 19); and the like. On the other hand almost equal use is made of the introspective and objective methods, with a bit more confidence placed in the objective approach. Also there is no consistent effort to show in detail how the concepts of an introspective psychology are translated into behavioristic terminology. In this way the text is more nearly functional in character and would, therefore, lend itself readily to that type of teaching approach.

The book is monistic—almost repetitiously so in the first two chapters—in its denial of a special mental or mind substance (p. 5), stating that mental processes belong in the "realm of the natural order" (p. 4); that there is "no mental stuff that is different from the other stuff of the universe" (p. 4); and that "there is no evidence of any special kind of energy, mental or otherwise, nor is there any need for postulating it in order to explain behavior" (p. 26). It may be recognized that the begin-

ning student comes to the study of psychology with a bias in the direction of a dualism (illy-formed though it may be) and hence it may be well for an emphasis to be placed on a monism if that be the author's position. It might, however, be suggested that the force of the argument may be lost unless there is given a clearer statement of the implications of a dualism than that in the text under review.

The book "introduces more anthropological material than do most of the introductory texts in the field" (Preface, p. vii). This material is introduced in the third and fourth chapters to emphasize the rôle of culture in directing and creating behavior patterns and in the formation of personality. The cultural is seen to bulk so large as a determinant of human behavior that culture is taken as the "most inclusive ground for differentiating man from animals. Thus we may say *that the ways of animals are instinctive while the ways of men are cultural*" (p. 78; italics author's).

Other sections and materials of the book are pretty largely conventional in nature. They are characterized by the liberal introduction of recent experimental results and by less emphasis upon older and classical experiments. Little complaint can be found with the factual material introduced save that in a few places some literature contemporaneous with that cited is omitted. In general the book is strongest in its presentation of pertinent data.

With reference to the suitability of the book as an introductory text, the reviewer ventures the following: The text will serve best those who are interested in a functional approach to psychology, in spite of the author's favoring the behavioristic tradition. In general the book utilizes to a great extent the language of more traditional usage, *i.e.* "instinct," "feeling," which will be troublesome in spots because of inevitable ambiguities which are not always avoided. If an instructor likes a text which does not avoid controversial questions even though the treatment of some of them may appear inadequate, a book which stresses recent experimental literature, a book which is crowded with detail, but still maintains an effort toward presenting psychology as a rigorous scientific discipline, the present text will be found satisfactory.

R. H. WATERS.

University of Arkansas.

KATZ, D. Animals and men: studies in comparative psychology. New York: Longmans, Green, 1937. Pp. xi+263.

The subject matter of this introduction to comparative psychology naturally falls into three divisions: (1) viewpoint and methodology; (2) experimental studies of sense perception and spatial orientation; behavioral expressions of needs, drives, and instincts; and social behavior; and (3) a psychological comparison of man and infrahuman animals.

In Chapter I the comparative point of view is stated and the accomplishments of those who hold a critical, problem-solving attitude as opposed to those who have a dogmatic viewpoint when interpreting facts of observation are held in contrast. Here Professor Katz makes good use of the pseudo accomplishments of Clever Hans and the Elberfeld

horses to exemplify the common errors of observation and inference committed by laymen and scientists who essay to plumb the minds of animals without proper orientation. Chapter II deals with some of the more important relations of animal psychology to the other biological sciences. One is convinced that the literature of all of the life sciences now is being augmented in important ways by fundamental work in animal psychology. The chapter ends with a brief section on *applied animal psychology*, one of the first of its kind to appear in a textbook. In Chapter III a few of the more important principles underlying methodology in human and animal psychology are presented. In this connection, the author stresses the need of unprejudiced description of the objects and circumstances of the experiments and advocates the utmost sincerity on the part of the experimenter in trying to catch the animal's point of view during the period of experimental observation. Qualitative observations have scientific value by way of giving preliminary orientation to the scientist in the formulation of plans for the solution of complex problems. No distinction need be made between the value of scientific field observations and scientific laboratory studies. One supplements the other, but the latter ofttimes is indispensable for the final clarification or quantification of details that cannot be elucidated by chance or rare observations in the field.

To American readers, the most valuable and interesting features of Chapters IV, V, VI, and VII may be the excellent accounts of animal studies from the Continental laboratories. Unfortunately, there has been a lag in the assimilation of these investigations by American reviewers and authors of textbooks. While quite frequently the Continental investigations are less adequate than the American studies from a statistical point of view, they tend, as a rule, to surpass our studies in two noteworthy characteristics, namely: their more obvious connection with fundamental psychological problems, and their more mature discussions of theoretical implications. To preserve the latter characteristics, a man of Dr. Katz's maturity, catholic interests, and superior grounding in experimental psychology is required.

In the final chapter, theoretical and experimental literature from animal psychology is fused with that of the corresponding fields of human psychology in such a way that the solution of problems in the one tend to illuminate the problems of the other.

In no essential way does this little book duplicate the subject matter of any other book in the field of animal psychology. It should find a place in the library of everyone interested in comparative psychology, for it is the best brief work now available on this subject.

CALVIN P. STONE.

Stanford University.

BRIDGMAN, P. W. The intelligent individual and society. New York: Macmillan, 1938. Pp. vi+305.

The intelligent individual is Mr. Bridgman, and in this "essay" he has set forth his personal philosophy concerning society. It would be

unfair to criticize the book rigorously, since the author makes only the modest claim in the Preface that "it says out loud all sorts of things that are perfectly obvious to anyone." He is seldom dogmatic and he happily attacks the very mysticism with which many experts in one field—but not Mr. Bridgman—usually approach other fields that lie beyond their technical interests. The writing is so cool and sincere that the exposition of a banality such as "Growing old should be an exciting intellectual adventure" is stimulating and not the least bit offensive. Psychologists can find nothing new in the chapter entitled "Human Traits," and they are requested to tolerate the initial distinction between the emotions and the intellect in the light of the succeeding discussion.

There is nothing new, moreover, in the entire book. And from this it follows not that the book should be dismissed with a sneer but that the creed of operationalism has nothing new to offer. *The intelligent individual and society,* consequently, ought to be read by those who have adopted the slogan of operationalism as a panacea for most of the methodological problems of psychology and also by those who appreciate and enjoy a sweeping, unbefuddled intellect.

Mr. Bridgman evidently is amazed at the interpretations his pseudo-followers have placed upon operationalism:

> "The operations which are most obviously characteristic of physics are to a large extent operations with our hands—the sort of thing we do in the laboratory in making measurements. But it is impossible to separate the things we do with our hands from the things we do with our 'minds', for it is obvious that there are 'mental' operations inextricably mixed up with such a primarily physical thing as measuring a length, and of course there are also operations in physics which are almost entirely 'mental', as for example counting, which the physicist, no less than the financier, could not get along without. . . My former usage would not at all have excluded such things as the verbal operations which now concern us, and in fact I have often referred to 'mental' operations, but since our concern was primarily the validity of our concepts in the 'external' physical world, we usually had to demand that our verbal regresses could be made to end in some physical operation. The right answer could in many cases have been obtained if we had restricted the meaning of 'operation' to 'physical operation', and in fact a good many of my readers apparently have had the erroneous idea that 'operation' *had* to be understood in this restricted sense. The necessity of a wider meaning is now more evident than before" (pp. 46, 89).

The moment "verbal operations" are considered worthy scientific pursuits all the shouting about immediate measurement must subside and operationalism becomes a logical, empirical analysis of social and psychological events in a manner previously discovered by anyone with scientific training and horse sense. "Our analysis of social concepts will take the form of analyzing what we do when we apply the concept to concrete situations." When this is done, the words that help to control men's social lives are stripped of their mysticism in what Mr. Bridgman calls a "rational" fashion. "There probably is more bunk connected with the concept of the state than with any other social concept." Operationalism in the social sciences, then, is about equivalent to the muckraking of

the "group mind" by F. H. Allport and the peace societies or the modern popular game of tracking down symbols as engaged in by T. Arnold and S. Chase. Indeed, the lucid dissection of duty, freedom of the will, rights, truthfulness, and other standardized clichés is the product of Mr. Bridgman's own brilliance as well as the extension of a method he employs in physics.

Operationalism or, if the old-fashioned appellation is permitted, the scientific attitude has at least three important consequences that Mr. Bridgman discusses at length. One he calls "the great limitations"—limitations placed upon all clear thinking by man's nature and the epistemological and psychological difficulties which confront that nature. Another is the need for denotative definitions and quantification, in which respects it is admitted that the social sciences, including psychology, are still relatively immature. That connotative definitions in any discipline eventually must be bolstered with denotatively defined concepts, however, is a point that was made by most elementary logic textbooks before intelligence tests ever tested what they tested.

Education and science, moreover, as Mr. Bridgman shows, tend to destroy "the great social fictions of our day." Thereafter, guides for personal and social conduct have to be sought. This particular author, after debunking himself as well as he can, arrives at a solution for the individual that is a combination of the biblical golden rule and the Kantian categorical imperative. He is forced to admit, furthermore, that there "seems to be no automatic method by which the rationality of an institution gets assured, even with infinite time and widely fluctuating conditions in which to bring about the change." He continues: "The only method which I have observed to be effective in the production of rationality is deliberate criticism on the part of members of the community and consequent self-conscious modification of their actions." It is gratifying to find Mr. Bridgman adhering to this democratic conclusion by denouncing totalitarianism so vigorously. Here is not the place, however, to agree or disagree with the rational life he wishes to establish for himself and his society. It is sufficient to suggest that, operationally, Mr. Bridgman has had to manipulate an old and significant theme: no conscientious and thoughtful scientist who applies his science to the people and events that surround him can avoid the problem of value and the rôle of science in social affairs.

LEONARD W. DOOB.

Yale University.

PRESCOTT, D. A. Emotion and the educative process. Washington, D. C.: American Council on Education, 1938. Pp. xviii+323.

Emotion and the educative process was prepared by the Committee on the Relation of Emotion to the Educative Process, a subcommittee of the Problems and Plans Committee of the American Council on Education. Dr. Prescott was chairman of the subcommittee; Dr. Frederick H. Lund, secretary; and other members were John H. MacCracken, James S.

Plant, Edward Sapir, George D. Stoddard, Vivian T. Thayer, and M. Ernest Townsend.

The purpose of the investigation was to bring to the attention of educators and those interested in education the pressing character of the problem of human emotion in the developmental and adjustment processes. In its letter of transmittal to President Zook of the Council on Education, it well points out that world political developments, propaganda, mob action, and impatience with the scientific approach to national problems have "driven home the lesson that the job of education is not done when knowledge is disseminated and increased. If the scholar, concerned with his primary business of knowledge, fails to deal with the whole man, particularly with the control of passion and the guidance of desire, he may properly be charged with contributory negligence when the democracy becomes either a mob or a regimented army, when freedom to learn or to teach disappears, when the neglected emotions submerge the life of reason, and so force recognition of their claim to a share in the lives of men."

We are reminded at the outset of the paucity of facts about "emotional maturity." To what extent does emotional maturity mean de-emotionalization and inhibition? Does education result in undesirable frustration and hence in lack of adjustability of the individual? We are reminded also that psychologists still disagree on what the basic affective phenomena are. The book has rendered a good service to psychology and education in its fairly thorough epitome of contemporary points of view, together with results from many of the more recent experimental studies. These studies, however, do not adequately answer the questions: Are there both general and specific emotional attitudes, some of which will transpose from one situation to another, some of which will not? Which type, if either, dominates in the life of the individual? These questions need sadly to be answered in the interests of mental hygiene and social stability. The further question arises: To what extent do intellectual processes stabilize the emotions and hence lead either to moral or immoral behavior? These questions are discussed, leading up to the integration of the self. In this connection, the work of Dr. Lewin is given a deserved emphasis.

There follows a review of the physiological bases of affective behavior in which the results of recent investigations by Child, Huxley, Coghill, Lashley, Cannon, Bard, and others are emphasized. The importance of physiological gradients for emotional stability and lability is recognized.

Next comes the problem of patterning and trainability of affective behavior. Are there innate patterns? Does maturation determine, or at least contribute to, emotional growth and fixation? The modern point of view is accepted: "Maturation influences the expression of emotion by the development of capacities rather than the ripening of specific innate response patterns." Here, stress is laid on K. M. B. Bridge's concept of the gradual differentiation of behavior patterns, on Goodenough's developmental point of view, on Luria's finding that "affective

behavior is not patterned with regard to stimulation, but varies according to the functional situation," and on Lewin's dynamic theory of emotion. Then, certain conclusions for education are drawn as, for example:

"The understanding that pattern of emotional behavior is so greatly a matter of experience should have a very positive effect upon the objectives of education. . . . Emotions and strong feelings are not things to be feared and avoided—they have their roots in biological utility. . . . Courageous experimentation is needed until educational curricula are found which will lower markedly the incidence of delinquent and pathological behavior. . . . A hunch suggests that schools must offer a challenge to children rather than the 'soft' pedagogy of letting them do as they wish."

There follows a well-ordered discussion of basic personality needs and the conditions which frustrate them, and then a discussion of affective behavior and contemporary social institutions. Among the effects of contemporary society upon the growing child are overstimulation, increase of wants, uprootedness, insecurity, growth of competition, undesirable labor conditions, delinquency and crime, ethnic problems, and last but not least, international anarchy. Schools do not adequately lead the older pupils to examine critically the social situations about them. As a result they become emotionally overexcited on the one hand or lethargic on the other with respect to social relations. The growing child should acquire a perspective of social changes so that he may expect further change and form some opinion about the direction of the expected change. In this connection the committee might have made their case much more effective, in the reviewer's opinion, by including a chapter on the emotional life of nations and races as wholes, particularly in periods of stress or cultural change.

Next, there is a discussion of affective factors in relation to learning. Here the discussion is disappointing. It slights the obvious dependence in most learning situations of the feelings on degree of insight. Timely, however, is the admonition that rote learning be reduced to a minimum and that the evaluation of pupil progress be made in terms of personality development first, and in terms of specific knowledges and skills second.

The last part of the book is concerned with aspects of education needing study from the standpoint of the affective life. The deplorable condition of the curriculum is emphasized, but numerous recent procedures aiming to alleviate the situation are barely touched upon. The reader might have been referred to more of the extensive literature, a review of which would have been out of place in the present volume. And finally, the book ends with a discussion of personnel problems in education as they bear upon the life of the child. The occupational insecurity of the teacher, the frequent lack of rapport between teachers and supervisors, the increase of teacher insecurity by pressure groups, the need for strong professional organizations, and the problem of the personally maladjusted teacher, are all reviewed and could have been stressed even more vigorously.

One is somewhat disappointed with the final chapter on general conclusions, although one can accept its point of view wholeheartedly as far

as it goes. An excellent perspective is obtained from the book as a whole. It is reliable and modern. "Improving the mental hygiene of the schools is not an end which can be accomplished by educators working alone nor yet by psychiatrists through the multiplication of clinics. Perhaps we have here another test of democracy, which demands unselfish and often intangible contributions from many people for the common good."

RAYMOND H. WHEELER.

University of Kansas.

LAPIERE, R. T. Collective behavior. New York: McGraw-Hill, 1938. Pp. xiii+577.

In introducing this book the author states:

"So far, sociopsychological attention has been focused on the effects of social interaction upon the personalities of the individuals involved. The results of such attention have been amazingly fruitful, and much is now known concerning the processes by which individuals develop their personalities. Little has been done, however, with that aspect of social psychology which involves consideration of social interactions themselves. . . Data have been accumulating here and there, data which are often conflicting, unrelated, and incomplete but which are in the mass nevertheless impressive. It has been my aim to bring these data together, to supplement them with the results of general observation, and to build therefrom a tentative frame of reference for further study" (p. v).

The analysis of social interaction proceeds on the basis of five indices: the origin and function of the interaction; its ideologies; the membership of the situation; the relation between the overt behavior of the members and their covert feeling states; the personnel, the character, and the rôle of leadership. On the basis of these indices, four general types of interaction are discussed: (1) cultural interaction, which includes chapters on institutional, conventional, regimental, and formal behavior; (2) recreational interaction, with chapters on congenial behavior, fugitive patterns, audience behavior, types of audiences, public behavior, and recreational aspects of leadership; (3) control types of interaction, which include exchange, politic, and nomothetic behavior; and (4) escape types of interaction, under which panic, revelous, fanatic, and rebellious behavior are discussed.

Appended to each chapter are several pages of notes and bibliographies. For his illustrative material from primitive society, the author selected as his main source Thomas' *Primitive behavior;* and from contemporary society "whenever possible," *Time* magazine (p. 59). Liberal use is made of the *Encyclopedia of the social sciences,* with occasional citations from articles in *Harper's, Saturday Evening Post,* and *Collier's.*

As justification of his dependence upon *Time* for contemporary material, the author states that *Time* has been criticized by radicals, conservatives, fascists, anti-fascists, capital, labor, and other opposed groups. "*Time* is a commercial enterprise. But any news organ which can be so roundly condemned by spokesmen of all factions and which can be so

consistently read as *Time* must certainly have in rare degree that exceptional bias of impartiality" (p. 60).

Why a publication which has at some time or other been condemned by differing factions "must certainly" have a rare impartiality is not explained. Can the truth of a news story be said to be established because people of different opinions deny its truth? That thousands subscribe to *Time* can prove nothing whatever concerning the accuracy of its reporting. Objective evaluations of magazines are available in several studies,[1] which do not promote the conviction that LaPiere's chief source of contemporary sociopsychological material was the most adequate one available. This point would hardly be worth elaboration except that it is not an atypical sample of the reasoning and evidence which characterize enough of the book to impair rather seriously its value as a whole.

In discussing the ideologies of "politic" behavior, the author states:

> "In the solving of problems, however, as distinct from the simple answering of questions, two heads are certainly not better than one. The solving of any problem, whether it be the problem of designing a building or of forming a plan of action for a business, religious, educational, or other organization, is a matter of individual trial-and-error. Collective interaction in the trial-and-error efforts to solve a problem is likely to result far more in utter confusion than in a solution to that problem. It is as reasonable to assume that ten rats turned loose in a maze will, through interaction with one another, solve the maze problem more efficiently than would any one of them interacting with the maze itself as to assume that ten men discussing a problem will arrive at a solution more efficiently than will one of them working on that problem. . . Collective solving of any problem is per se a sociopsychological absurdity" (pp. 377–378).

It is not made clear how such a sweeping generalization is to be reconciled with G. B. Watson's results and the analysis of group thinking in Murphy, Murphy, and Newcomb's, *Experimental social psychology*, for example. Watson's experiments and others are listed in a bibliography, but no discussion of actual experimental results is given.

The author criticizes LeBon's analysis of crowd behavior as being "little short of Hegelian," but then states, "His (LeBon's) central thesis, that an individual behaves differently in a group than he does in isolation, is incontestable" (p. 545). Evidence in support of this rather categorical statement is not given. The experiments of Hamilton and Patrick seem to indicate that the isolated individual is capable of fairly extreme irrational behavior. Informal evidence concerning the behavior of individuals in burning buildings and the like allows room for doubting that qualitatively unique behavior is elicited in crowds. Space forbids citation of other generalizations stated without qualification by the author—universal positive and negative propositions have an incongruous appearance in a treatise on social science.

LaPiere's contribution lies in his bringing together a large amount of material and providing useful starting points for its analysis. But

[1] Cf., for instance, Eldridge, S. Public intelligence. Lawrence: Univ. Kansas Press, 1935.

although the book is "more analytical than descriptive" (p. vi), the pursuit of fundamental factors determining social interactions is, in most chapters, rather short-lived. No attempt is made to discover principles permitting of deductions capable of empirical verification, nor is there any discussion of such attempts by others.

If attempts at analysis were to be made they might be expected to appear in the sections on the origins of interactional patterns. But in the author's discussion of the origin of "Exchange Behavior," for instance, the matter is disposed of substantially as follows: "The sociological origins of an exchange situation are complex and difficult to interpret . . . behind most modern exchange situations lie the complex historical factors which led to the disintegration of European feudalism and to the commercial and industrial revolutions" (pp. 341–342). As for the origin of "Nomothetic Behavior," "The sociological antecedents of any nomothetic situation are exceedingly complex, and those of no two such situations are quite alike. Furthermore, our knowledge of those antecedents may be exceedingly limited . . ." (p. 406). The origin of "Revelous Behavior" lies in "psychological tensions which are brought about by the maladjustment of the individual to his socially designated role . . . The ultimate origins of social maladjustment are, therefore, sociological in character. The immediate consequences of such maladjustment are, however, psychological in nature" (p. 463).

It is quite true, of course, that in a general treatment of the entire gamut of social interaction, compressed into one volume of average length, it is probably impossible to make an analysis complete enough to yield principles which will promote understanding in a fundamental sense. At the same time, such a treatment lays itself open to the charge of superficiality, and in the present case may hardly be called more analytic than descriptive. The author states, in regard to the uses for which the book is intended, "Pedagogically, I think of this field of inquiry, whatever it may be called, as the second course in social psychology" (p. vi). It is doubted that psychologists will find the book appropriate for courses in advanced social psychology.

JOHN W. MCGARVEY.

Mount Holyoke College.

ELLWOOD, C. A. A history of social philosophy. New York: Prentice-Hall, 1938. Pp. xiv+581.

Here is an able survey of the major trends in social thinking, of "what man has thought regarding . . . his institutions, his culture, and the meaning of his history." Except for a page or two at the beginning, the exposition is limited to the philosophies of western European civilization. The emphasis is sociological; Comte, Spencer, William Sumner, and Lester Ward occupy nearly a fourth of the book.

There are three parts. The first (272 pp.) concerns the "precursors of sociology"; it begins with a short chapter on primitive and early Greek thought and ends with the British philosophy of the late Eighteenth Century. The second part (70 pp.) contains an account and criticism of

the so-called "one-sided" viewpoints, presented under the five headings: ideological, individualistic, racial, geographical, and economic social philosophies. The third part (218 pp.) covers the leading sociological thinkers from Saint Simon to Lester Ward.

The organization is clear and, considering the heterogeneity of material, remarkably uniform. Each man is introduced with a brief biographical account because of "the obvious importance of unique biographical incidents in determining thinking." There follow in order, wherever the philosopher's writings permit, sections on scientific method, social origins, social development, social organization, social order, and social progress. A page or two of criticism completes each section.

The style is easy and lucid. Rarely does one find abstract ideas expressed so comprehensibly. Undergraduates should have no difficulty with this book. The criticisms are eclectic in tone and show knowledge, reflection, and judgment. The reviewer is surprised at the paucity of his exceptions to the author's comments, in view of the admittedly controversial nature of much social thinking.

HERBERT GURNEE.

Western Reserve University.

STRONG, E. K., JR. Psychological aspects of business. McGraw-Hill, 1938. Pp. x+629.

This book is a revision and expansion of the author's *Psychology of selling and advertising*, published in 1925. Strong outlines his main theme as "influencing others," which is more or less characteristic of most works on psychology in business or commerce. Strong's efforts are especially directed at those phases of influencing others which apply to selling, advertising, and managing men.

There are six parts, totaling thirty chapters. Part I, "General Survey," outlines the motives behind purchase and sale which are necessary to put them across. Part II deals with "How Man Satisfies His Wants," and is really a sample of general psychology, limited to those aspects which the author considers basic to more practical matters. This has been attempted previously by several authors, and the reviewer feels that most of such attempts have been rather disappointing. There is apt to be too much discrepancy between the theory and the practice. I feel that in this sort of book discussion of basic principles and theories behind concrete suggestions will mean more to the business man if introduced from time to time as the occasion arises rather than if they are grouped in a separate section.

Part III, "Principles of Influencing Others," and Part V, "Research Techniques," are the advertising sections. The chapters on "Consumer Research," "Appeals," and "Measurement of Advertisements" struck me as the most meaty of the thirteen chapters of these two parts.

In my opinion, the outstanding feature of Strong's work is the detailed discussion of person-to-person salesmanship, contained in Part IV, "Tactics of Selling," as well as in a few sections in other parts of the book. The principles apply especially to selling insurance or other pos-

sible services or commodities which are high priced, thus demanding long and careful planning to guide the interview and to break down customer resistance. In fact I believe that every such salesman (in contrast to the salesman in the retail store) could read and reread with great profit these sections, and keep the book on his desk for constant reference. Strong shows how such an interview is set up in terms of motives of the prospect, how possible objections are to be overcome or circumvented, how the most suitable solution is chosen, and how the whole situation is synthesized to achieve the desired results.

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DRAKE, R. M. Outline of abnormal psychology. New York: Longmans, Green, 1937. Pp. 136.

No conscientious teacher of abnormal psychology needs a reminder that the course is difficult to organize. Either from force of habit or as a matter of convenience most departments relegate to it a number of topics which bear no close systematic relation to one another and which, historically, have been studied from very different systematic positions. The organic disorders of locomotion are, in a sense, disturbances or abnormalities of the psychological function of *action* and hypnosis is at least an unusual psychological phenomenon, but to get them both into one course without having the syllabus seem a veritable catalogue of unrelated facts is difficult.

To meet this situation, Dr. Drake, in this paper-bound outline, has resorted to much the same organization used so successfully in textbooks by Morgan, Conklin, and Dorcus and Shaffer. The chapter headings include an "Introduction," "Sensory and Perceptual Disorders," "Motor Disorders — Speech Disorders," "Association Mechanism," "Memory Abnormalities," "Desires," "Feelings and Emotions," "Intellectual Abnormalities," "Psychoneuroses," "Organic Psychoses," "Functional Psychoses," "Sleep and Its Abnormalities," "Dreams," "Hypnosis and Suggestibility," "Psychotherapy," and "Spiritistic Phenomena." The systematization of the material lies in the manner of presentation within each chapter; definitions and facts are given first, and are followed by extensive theoretical explanations with careful comparisons of the various schools of thought on each topic. The outline is admirable in its completeness and lack of bias. It could be used with profit as a syllabus to accompany any of the standard textbooks. The textual material (in the best outline format) is printed on only one side of each page, thereby facilitating the taking of class notes.

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BOOKS RECEIVED

CURTI, M. W. Child psychology. (2nd ed.) New York: Longmans, Green, 1938. Pp. vii+458.

HARROWER, M. R. The psychologist at work: an introduction to experimental psychology. New York: Harper, 1938. Pp. xiv+184.

KAUFMAN, G. L. The book of time. New York: Messner, 1938. Pp. xvi+287.

KUHN, E. G. The pronunciation of vowel sounds: an evaluation of practice material for college freshmen. Teach. Coll. Contr. Educ., No. 757. New York: Bureau of Publications, Teachers College, Columbia Univ., 1938. Pp. vi+86.

MURRAY, H. A. Explorations in personality. New York: Oxford Univ. Press, 1938. Pp. xiv+761.

OGDEN, R. M. The psychology of art. New York: Scribner, 1938. Pp. xviii+291.

NOTES AND NEWS

THE Works Progress Administration has recently published an *Index of Research Projects,* which includes the results of around 2000 research projects carried on as part of the federal work relief program. Almost every field of natural and social science has been touched upon in these reports, several hundred of which are in manuscript form and may be obtained by research specialists in microfilm copy, at nominal rates. A limited supply of copies of this 291-page index and digest are available to those interested, and may be obtained upon request from the Works Progress Administration in Washington.

A NEURO-PSYCHIATRIC clinic building for the State Hospital for Mental Diseases at Providence, R. I., has been named for Dr. Adolf Meyer, Henry Phipps professor of psychiatry at the Johns Hopkins University School of Medicine. To mark the occasion a testimonial dinner was tendered to Dr. Meyer at the University Club, which was attended by a distinguished group of psychiatrists and neurologists. Dr. Meyer was characterized as "a wise, scientific leader of psychiatry, who has piloted the bark of this developing branch of medicine through its years of growth . . . avoiding the many pitfalls of new cults . . . always searching for scientific truth."—*Science.*

A new "Vocational Current Events Digest," entitled *Life and Careers,* has just been published by Life and Careers, 2222 West Seventh Street, Los Angeles, California. It constitutes a contribution by the Educational Research Association "to the work of guidance and vocational teachers for the interchange of occupational information of direct value to the students themselves." This digest of up-to-date vocational news may be obtained for 50 cents per school year, or 10 cents per copy, by writing to the above address.

IN THE Spring of 1939 a post-doctorate research fellow in psychical research will be appointed at Stanford University at a salary of $2500. The appointment will be for 1 year with the possibility of reappointment for a second, or even a third year. Inquiries should be addressed to Lewis M. Terman, Chairman, Department of Psychology, Stanford University, California.